ACTA COLL·MED·PHILAD·INSTIT·A·D·MDCCLXXXVII

C. F. Judson.

With this volume is bound the William F. Jenks Prize Essay of the College of Physicians of Philadelphia, on the "Diagnosis and Treatment of Extra-uterine Pregnancy," by John Strahan, M.D., M.Ch., M. A.O., of Belfast, Ireland.

Prize awarded January 29, 1889.

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TRANSACTIONS

OF THE

COLLEGE OF PHYSICIANS

OF PHILADELPHIA.

VOLUME THE EIGHTEENTH.

NOTICE.

THE present volume of TRANSACTIONS contains the papers read before the College from January, 1889, to December, 1889, inclusive.

The Committee of Publication thinks it proper to say that the Committee holds itself in no way responsible for the statements, reasonings, or opinions set forth in the various papers published in its Transactions.

[Volume XI. of the Transactions has been edited by Dr. J. P. CROZER GRIFFITH.]

TRANSACTIONS

OF THE

COLLEGE OF PHYSICIANS

OF

PHILADELPHIA.

THIRD SERIES.

VOLUME THE ELEVENTH.

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1889.

COLLEGE OF PHYSICIANS OF PHILADELPHIA.

1889.

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LIST

OF THE

PRESIDENTS OF THE COLLEGE FROM THE TIME OF ITS INSTITUTION.

ELECTED	
1787.	JOHN REDMAN, M.D.
1805.	WILLIAM SHIPPEN, M.D.
1809.	ADAM KUHN, M.D.
1818.	THOMAS PARKE, M.D.
1835.	THOMAS C. JAMES, M.D.
1835.	THOMAS T. HEWSON, M.D.
1848.	GEORGE B. WOOD, M.D., LL.D.
1879.	W. S. W. RUSCHENBERGER, M.D.
1883.	ALFRED STILLÉ, M.D., LL.D.
1884.	SAMUEL LEWIS, M.D.
1884.	J. M. DA COSTA, M.D., LL.D.
1886.	S. WEIR MITCHELL, M.D., LL.D.
1889.	D. HAYES AGNEW, M.D., LL.D.

FELLOWS

OF THE

COLLEGE OF PHYSICIANS OF PHILADELPHIA.

DECEMBER, 1889.

[Non-resident Fellows are marked (*).]

ELECTED

1883. Abbot, Griffith E., Ph.D., M.D.

1870. Adler, John M., M.D.

1859. Agnew, D. Hayes, M.D., LL.D., (Emeritus) Professor of Surgery in the University of Pennsylvania; Consulting Surgeon to the Orthopædic, the Maternity, and St. Christopher's Hospitals.

1876. Alison, Robert H., M.D.

1867. Allen, Harrison, M.D., Emeritus Professor of Physiology in the University of Pennsylvania.

1873. Allis, Oscar H., M.D., Clinical Lecturer on Orthopædic Surgery in the Jefferson Medical College and Surgeon to the Hospital of the same; Surgeon to the Presbyterian Hospital.

1888. Anders, James M., M.D., Professor of Hygiene and Clinical Diseases of Children in the Medico-Chirurgical College, Philadelphia; Physician to the Philadelphia Hospital.

1869. Andrews, T. Hollingsworth, M.D., Consulting Surgeon to the Hospital of the Good Shepherd, Radnor; Medical Director of the Bureaus of Police and Fire of the Department of Public Safety.

*1882. Ashbridge, Richard, M.D., Assistant Surgeon U. S. Navy.

ELECTED

1863. Ashhurst, John, Jr., M.D., Professor of Surgery in the University of Pennsylvania; Surgeon to the Pennsylvania and the Children's Hospitals; Consulting Surgeon to St. Christopher's and the Woman's Hospitals, and to the Hospital of the Good Shepherd, Radnor.

1865. Ashhurst, Samuel, M.D., Surgeon to the Children's Hospital.

1857. Atlee, Walter F., M.D., Consulting Physician and Surgeon to St. Luke's Hospital, Bethlehem.

1852. Bache, Thomas Hewson, M.D.

1883. Baer, Benjamin F., M.D., Professor of Gynecology in the Philadelphia Polyclinic.

1879. Baker, Washington H., M.D., Obstetrician to the Maternity Hospital.

1876. Baldwin, Louis K., M.D., Examining Physician to the Hospital of the Good Shepherd, Radnor.

1889. Baldy, John M., M.D.

1880. Bartholow, Roberts, M.D., Professor of Materia Medica, General Therapeutics, and Hygiene in the Jefferson Medical College.

1883. Baum, Charles, M.D.

1873. Baxter, H. F., M.D.

1883. Beates, Henry, M.D.

1860. Benner, Henry D., M.D.

1874. Bennett, W. H., M.D., Physician to St. Christopher's Hospital.

1884. Biddle, Alexander W., M.D.

1884. Biddle, Thomas, M.D.

*1866. Black, J. J., M.D.

*1867. Boardman, Charles H., M.D.

1859. Boker, Charles S., M.D., Surgeon to St. Joseph's Hospital.

1884. Bradford, Thomas Hewson, M.D., Physician to the Dispensary of the Children's Hospital and to the Gynecological Departments of the Pennsylvania and the Howard Hospitals.

1856. Brinton, John H., M.D., Professor of the Practice of Surgery and of Clinical Surgery in the Jefferson Medical College; Surgeon to St. Joseph's Hospital; Consulting Surgeon to the Southwestern Hospital of Philadelphia.

1887. Brubaker, Albert P., M.D., Professor of Physiology and

ELECTED

General Pathology in the Pennsylvania College of Dental Surgery; Demonstrator of Physiology in the Jefferson Medical College.

*1851. Bullock, William R., M.D.

1887. Bunting, Ross R., M.D.

1870. Burnett, Charles H., M.D., Aurist to the Presbyterian Hospital; Consulting Aurist to the Pennsylvania Institution for the Deaf and Dumb; Lecturer on Otology in the Woman's Medical College.

1886. Cadwalader, Charles E., M.D.

1885. Chapin, John B., M.D., Physician to the Pennsylvania Hospital for the Insane.

1880. Chapman, Henry C., M.D., Professor of the Institutes of Medicine and of Medical Jurisprudence in the Jefferson Medical College.

1868. Cheston, D. Murray, M.D.

1873. Clark, Leonardo S., M.D.

1872. Cleemann, Richard A., M.D.

*1842. Clymer, Meredith, M.D.

1871. Cohen, J. Solis, M.D., Consulting Physician to the German Hospital.

1888. Cohen, S. Solis, M.D., Professor of Clinical Medicine and of Applied Therapeutics in the Philadelphia Polyclinic Consulting Physician to the Jewish Hospital; Lecturer on Special Therapeutics in the Jefferson Medical College.

1866. Cruice, R. B., M.D., Surgeon to St. Joseph's Hospital.

1884. Curtin, R. G., M.D., Lecturer on Physical Diagnosis in the University of Pennsylvania; Assistant Physician to the University Hospital; Physician to the Philadelphia and the Presbyterian Hospitals.

1884. Da Costa, J. C., M.D., Gynecologist to the Jefferson Medical College Hospital.

1884. Da Costa, J. M., M.D., LL.D., Professor of the Principles and Practice of Medicine in the Jefferson Medical College; Physician to the Pennsylvania Hospital; Consulting Physician to the Children's Hospital and to the Northern Dispensary.

ELECTED

1887. Daland, Judson, M.D., Instructor in Clinical Medicine in the University of Pennsylvania and Assistant Physician to the Hospital of the same.

1859. Darrach, James, M.D., Consulting Surgeon to the Germantown Hospital.

1888. Davis, Edward P., M.D., Professor of Obstetrics and Diseases of Children in the Philadelphia Polyclinic; Demonstrator of Obstetrics in the Jefferson Medical College; Visiting Obstetrician to the Philadelphia Hospital.

1889. Davis, G. G., M.D., Assistant Surgeon to the Episcopal and Orthopædic Hospitals; Surgeon to the Dispensary of the Children's Hospital; Assistant Demonstrator of Surgery in the University of Pennsylvania.

1874. Deakyne, A. C., M.D.

*1870. Deal, L. J., M.D.

1887. Deaver, John B., M.D., Demonstrator of Anatomy and Lecturer on Topographical Anatomy in the University of Pennsylvania; Surgeon to the German and to St. Mary's Hospitals.

1885. Dercum, Francis X., M.D., Instructor in Nervous Diseases in the University of Pennsylvania; Neurologist to the Philadelphia Hospital.

1864. Downs, R. N., M.D.

1884. Drysdale, T. M., M.D., Consulting Gynecologist to the Hospital of the Medico-Chirurgical College.

1864. Duer, Edward L., M.D., Accoucheur to the Philadelphia Hospital; Surgeon to the Maternity Hospital; Visiting Physician to the Preston Retreat.

1871. Duhring, L. A., M.D., Clinical Professor of Skin Diseases to the Hospital of the University of Pennsylvania; Dermatologist to the Philadelphia Hospital; Consulting Physician to the Philadelphia Dispensary for Skin Diseases.

1881. Dulles, Charles Winslow, M.D.

1863. Dunglison, Richard J., M.D.

*1871. Dunglison, Thomas R., M.D.

1888. Dunn, Thomas D., M.D.

*1849. Dunott, Justus, M.D.

1860. Dunton, William R., M.D., Consulting Physician to the Germantown Hospital.

ELECTED

1882. Edwards, Joseph F., M.D.
*1887. Edwards, William A., M.D.
*1880. Eskridge, J. T., M.D.
1868. Evans, Horace Y., M.D., Physician to the Charity Hospital.

1884. Fenton, Thomas H., M.D.
1866. Fischer, Emil, M.D.
1884. Fisher, Henry M., M.D., Physician to the Episcopal Hospital; Microscopist to the Pennsylvania Hospital and Physician to the Out-patient Department.
1888. Flick, Lawrence F., M.D.
1862. Forbes, William S., M.D., Professor of Anatomy in the Jefferson Medical College.
1870. Ford, W. H., M.D.
1884. Formad, H. F., M.D., Lecturer on Experimental Pathology and Demonstrator of Morbid Anatomy in the University of Pennsylvania; Pathologist to the Philadelphia Hospital.
1885. Fox, Joseph M., M.D., Surgeon to the Out-patient Department of the Pennsylvania and the Children's Hospitals.
1885. Fricke, Albert, M.D.
1889. Fussell, M. Howard, M.D., Physician to the Medical Dispensary of the University of Pennsylvania.

1873. Gerhard, George S., M.D.
1884. Getchell, F. H., M.D.
1885. Girvin, Robert M., M.D., Gynecologist to the Presbyterian Hospital.
1889. Githens, William H., M.D.
1884. Godey, Harry, M.D.
1868. Goodell, William, M.D., Professor of Clinical Gynecology in the University of Pennsylvania; Consulting Physician to the Lying-in Department of the Northern Dispensary.
1867. Goodman, H. Earnest, M.D., Professor of Surgery in the Medico-Chirurgical College; Surgeon to the Wills and the Orthopædic Hospitals; Consulting Surgeon to the Maternity Hospital.
1885. Graham, John, M.D.
1870. Grier, M. J., M.D.
1883. Griffith, J. P. Crozer, M.D., Instructor in Clinical Medicine in the University of Pennsylvania, and Assistant Physician

ELECTED

to the Hospital; Physician to St. Agnes' and the Howard Hospitals; Pathologist to the Presbyterian Hospital.

1842. GRISCOM, JOHN D., M.D.

1883. GROSS, FERDINAND H., M.D., Surgeon to the German Hospital.

1871. GROVE, JOHN H., M.D., Surgeon to St. Mary's and to St. Agnes' Hospitals.

1889. GUITÉRAS, JOHN, M. D., Professor of General Pathology and Morbid Anatomy in the University of Pennsylvania.

1863. HALL, A. DOUGLASS, M.D., Surgeon to Wills Eye Hospital.

*1859. HAMMOND, WILLIAM A., M.D., Surgeon-General U. S. A. Retired.

1886. HANSELL, HOWARD F., M.D., Chief Clinical Assistant to the Ophthalmological Department of the Jefferson Medical College Hospital; Ophthalmic and Aural Surgeon to the Southwestern Hospital.

1889. HARE, HOBART A., M.D., Demonstrator of Therapeutics and Instructor in Physical Diagnosis and in Physiology in the University of Pennsylvania; Physician to St. Agnes' Hospital and to the Children's Dispensary of the University Hospital.

1865. HARLAN, GEORGE C., M.D., Surgeon to Wills Eye Hospital and to the Eye and Ear Department of the Pennsylvania Hospital.

1863. HARLOW, LEWIS D., M.D.

1862. HARRIS, ROBERT P., M.D.

1885. HARTE, RICHARD H., M.D., Demonstrator of Osteology in the University of Pennsylvania and Assistant Surgeon to the Hospital; Surgeon to the Out-patient Department of the Pennsylvania Hospital.

1851. HARTSHORNE, HENRY, M.D., LL.D.

1888. HARTZELL, MILTON B., M.D., Pathologist to the Presbyterian Hospital; Assistant Physician to the Dispensary for Skin Diseases, University of Pennsylvania.

*1849. HASTINGS, JOHN, M.D.

1872. HAYS, I. MINIS, M.D.

1882. HEARN, JOSEPH, M.D., Surgeon to the Hospital of the Jefferson Medical College and to the Philadelphia Hospital.

1884. HENRY, FREDERICK P., M.D., Physician to the Jefferson Medical College and the Philadelphia Hospitals.

ELECTED

1872. Hinkle, A. G. B., M.D.

1888. Hirsh, Abram B., M.D., Adjunct Professor of General and Orthopædic Surgery to the Philadelphia Polyclinic.

1888. Hirst, Barton Cooke, M.D., Associate Professor of Obstetrics in the University of Pennsylvania; Gynecologist to the Orthopædic Hospital; Obstetrician to the Philadelphia and Maternity Hospitals.

1885. Holland, James W., M.D., Professor of Medical Chemistry and Toxicology in the Jefferson Medical College.

1879. Hopkins, William Barton, M.D., Surgeon to the Episcopal Hospital and to the Out-patient Department of the Pennsylvania Hospital.

1867. Horn, George H., M.D.

1888. Horwitz, Orville, M.D., Chief Clinical Assistant in the Surgical Dispensary of the Jefferson Medical College Hospital.

1884. Horwitz, Phineas J., M.D.

1868. Howell, Samuel B., M.D.

1881. Huidekoper, Rush Shippen, M.D.

1884. Hunt, J. Gibbons, M.D.

1854. Hunt, William, M.D., Surgeon to the Pennsylvania and the Orthopædic Hospitals.

1863. Hutchinson, James H., M.D., Physician to the Pennsylvania and the Children's Hospitals; Consulting Physician to the Woman's Hospital.

1871. Ingham, James V., M.D.

1885. Jackson, Edward, M.D., Professor of Diseases of the Eye in the Philadelphia Polyclinic.

1887. Jayne, Horace, M.D., Professor of Vertebrate Morphology in the Biological Department of the University of Pennsylvania.

*1864. Jones, S. P., M.D.

1885. Judd, Leonardo Da Vinci, M.D.

1867. Judson, Oliver A., M.D.

1886. Jurist, Louis, M.D., Chief of Throat Department in the Jefferson Medical College Hospital.

1877. Keating, John M., M.D., Obstetrician to the Philadelphia Hospital; Physician to St. Joseph's and the Howard Hospitals.

ELECTED

1849. KEATING, WILLIAM V., M.D., Physician to St. Joseph's Hospital.

1867. KEEN, WILLIAM W., M.D., Professor of the Principles of Surgery and of Clinical Surgery in the Jefferson Medical College.

*1887. KELLY, HOWARD A., M.D., Associate Professor of Gynecology in the Johns Hopkins University, and Gynecologist and Obstetrician to the Hospital.

*1844. KING, CHARLES R., M.D.

1875. KIRKBRIDE, JOSEPH J., M.D.

*1865. LA ROCHE, C. PERCY, M.D.

1887. LEAMAN, HENRY, M.D.

1883. LEFFMANN, HENRY, M.D., Professor of Chemistry in the Philadelphia Polyclinic and in the Woman's Medical College; Pathological Chemist to the Jefferson Medical College Hospital.

1851. LEIDY, JOSEPH, M.D., Professor of Anatomy in the University of Pennsylvania.

1885. LEIDY, PHILIP, M.D.

1855. LEWIS, FRANCIS W., M.D.

1877. LEWIS, MORRIS J., M.D., Physician to the Children's Hospital and to the Orthopædic Hospital and Infirmary for Nervous Diseases.

1849. LEWIS, SAMUEL, M.D.

1886. LLOYD, J. HENDRIE, M.D., Instructor in Electro-therapeutics in the University of Pennsylvania; Physician-in-charge of Home for Crippled Children; Neurologist to the Philadelphia Hospital.

1877. LONGSTRETH, MORRIS, M.D., Lecturer on Pathological Anatomy in the Jefferson Medical College; Physician and Pathologist to the Pennsylvania Hospital.

1886. MACCOY, ALEXANDER, M.D., Professor of Diseases of the Throat and Nose in the Philadelphia Polyclinic; Lecturer on Diseases of the Throat and Nose in the Woman's Medical College of Philadelphia.

1875. MCCLELLAN, GEORGE, M.D., Surgeon to the Philadelphia and the Howard Hospitals.

ELECTED

1871. McFerran, J. A., M.D.

*1885. Mallet, John W., M.D.

1889. Martin, Edward, M.D., Surgeon to the Philadelphia and the Howard Hospitals; Instructor in Operative Surgery in the University of Pennsylvania.

1887. Massey, Isaac, M.D., Surgeon to the Pennsylvania Railroad.

*1850. Mayer, Edward R., M.D.

1885. Mays, Thomas J., M.D., Professor of Diseases of the Chest and of Experimental Therapeutics in the Philadelphia Polyclinic.

1868. Mears, J. Ewing, M.D., Professor of Anatomy and Clinical Surgery in the Pennsylvania College of Dental Surgery; Demonstrator of Surgery in the Jefferson Medical College, and Gynecologist to the Hospital of the same; Surgeon to St. Mary's Hospital.

1875. Meigs, Arthur V., M.D., Physician to the Pennsylvania and the Children's Hospitals; Consulting Physician to the Pennsylvania Institution for the Instruction of the Blind.

*1884. Mifflin, Houston, M.D.

1881. Mills, Charles K., M.D., Professor of Diseases of the Mind and Nervous System in the Philadelphia Polyclinic; Lecturer on Mental Diseases in the University of Pennsylvania, and on Nervous Diseases in the Woman's Medical College; Neurologist to the Philadelphia Hospital, and Consulting Physician to the Department for the Insane of the Philadelphia Hospital.

1888. Mitchell, John K., M.D., Instructor in Clinical Medicine in the University of Pennsylvania; Attending Physician to St. Mary's Hospital; Assistant Physician to the University Hospital and to the Infirmary for Nervous Diseases.

1856. Mitchell, S. Weir, M.D., Professor of Diseases of the Mind and Nervous Diseases in the Philadelphia Polyclinic; Physician to the Orthopædic Hospital and Infirmary for Nervous Diseases; Consulting Physician to the Maternity Hospital.

1882. Montgomery, Edward E., M.D., Professor of Gynecology and of Clinical Gynecology in the Medico-Chirurgical College; Obstetrician to the Philadelphia Hospital.

1863. Morehouse, George R., M.D., Physician to St. Joseph's Hospital.

ELECTED

1886. Morris, Caspar, M.D., Physician to the Episcopal Hospital and to the Out-patient Department of the Pennsylvania Hospital.

1883. Morris, Henry, M.D., Gynecologist to the Howard Hospital.

1856. Morris, J. Cheston, M.D.

1861. Morton, Thomas G., M.D., Surgeon to the Pennsylvania and the Orthopædic Hospitals; Consulting Surgeon to the Jewish Hospital; Emeritus Surgeon to Wills Eye Hospital.

1864. Moss, William, M.D.

1882. Musser, John H., M.D., Assistant Professor of Clinical Medicine in the University of Pennsylvania; Physician to the Philadelphia Hospital.

1886. Neff, Joseph F., M.D.

1877. Neilson, Thomas Rundle, M.D., Surgeon to the Episcopal Hospital; Instructor in Genito-Urinary Diseases and Assistant Demonstrator of Anatomy in the University of Pennsylvania, and Chief of the Venereal Dispensary in the Hospital of the same.

1889. Noble, Charles P., M.D., Surgeon-in-chief to the Kensington Hospital for Women; Surgeon-in-charge of the Department for Women of the Northern Dispensary.

1869. Norris, Herbert, M.D., Supervising Physician to St. Clement's Hospital.

1865. Norris, Isaac, Jr., M.D.

1866. Norris, William F., M.D., Honorary Professor of Ophthalmology and Clinical Professor of Diseases of the Eye in the University of Pennsylvania; Surgeon to Wills Eye Hospital.

1884. Oliver, Charles A., M.D., Ophthalmic Surgeon to St. Agnes', St. Mary's, and the Maternity Hospitals; Visiting Physician and Ophthalmologist to the State Hospital for the Insane at Norristown.

1884. O'Neill, J. W., M.D.

*1885. Osler, William, M.D., Professor of Medicine in the Johns Hopkins University, and Physician to the Hospital.

1858. Packard, John H., M.D., Surgeon to the Pennsylvania and St. Joseph's Hospitals.

ELECTED

1864. PANCOAST, WILLIAM H., M.D., Professor of Anatomy and of Clinical Surgery in the Medico-Chirurgical College; Consulting Surgeon to the Philadelphia Hospital for Skin Diseases.

1882. PARISH, WILLIAM H., M.D., Professor of Anatomy in the Woman's Medical College; Obstetrician to the Philadelphia Hospital.

*1854. PARRISH, JOSEPH, M.D.

1883. PARVIN, THEOPHILUS, M.D., Professor of Obstetrics and Diseases of Women and Children in the Jefferson Medical College; Obstetrician to the Philadelphia Hospital.

1889. PENROSE, CHARLES BINGHAM, M.D., Surgeon to the Gynecean Hospital; Surgeon to the Out-patient Department of the Pennsylvania and St. Agnes' Hospitals; Assistant Surgeon to the University Hospital.

1854. PENROSE, R. A. F., M.D., LL.D., Emeritus Professor of Obstetrics and Diseases of Women and Children in the University of Pennsylvania; Consulting Obstetrician to the Maternity Hospital; Visiting Physician to the Preston Retreat.

1868. PEPPER, WILLIAM, M.D., LL.D., Provost of the University of Pennsylvania and Professor of the Theory and Practice of Medicine in the same.

1884. PERKINS, FRANCIS M., M.D., Ophthalmic and Aural Surgeon to the Dispensary of St. Mary's Hospital; Visiting Ophthalmic Surgeon to the Hospital of the Good Shepherd at Radnor.

1883. PIERSOL, GEORGE A., M.D., Professor of Histology and Embryology in the University of Pennsylvania.

1872. PORTER, WILLIAM G., M.D., Surgeon to the Presbyterian and the Philadelphia Hospitals.

1885. POTTER, THOMAS C., M.D.

1887. PRICE, JACOB, M.D.

1889. PRICE, JOSEPH, M.D., Surgeon to the Gynecean Hospital; Physician-in-charge of the Preston Retreat and of the Female Department of the Philadelphia Dispensary.

1889. RANDALL, B. ALEXANDER, M.D., Professor of Diseases of the Ear in the Philadelphia Polyclinic; Ophthalmic and Aural Surgeon to the Episcopal and Children's Hospitals.

ELECTED

1887. Reed, Charles H., M.D.

1866. Reed, Thomas B., M.D., Surgeon to the Presbyterian Hospital.

1842. Reese, John J., M.D., Professor of Medical Jurisprudence in the University of Pennsylvania.

1885. Reichert, Edward T., M.D., Professor of Physiology in the University of Pennsylvania.

*1884. Rex, Francis M., M.D.

1888. Rex, George A., M.D.

1883. Rex, Oliver P., M.D., Clinical Lecturer on Diseases of Children in the Jefferson Medical College, and Physician to the Hospital; Physician to the Presbyterian Hospital.

*1857. Richardson, Tobias G., M.D.

1882. Roberts, A. Sydney, M.D.

1878. Roberts, John B., M.D., Professor of Anatomy and Surgery in the Philadelphia Polyclinic; Professor of Surgery in the Woman's Medical College of Pennsylvania; Lecturer on Anatomy in the University of Pennsylvania; Consulting Surgeon to the Jewish Hospital; Surgeon to St. Agnes' Hospital.

1888. Robins, Robert P., M.D., Visiting Physician to the Dispensary of the House of Industry, to the Church Home for Children, and to the Board of Guardians of the Poor; Lecturer on Chemistry in the Episcopal Academy.

1843. Rodman, Lewis, M.D., Visiting Physician to the Preston Retreat.

1838. Ruschenberger, W. S. W., M.D., Medical Director U. S. N.

*1852. Sargent, Fitz William, M.D.

*1864. Sargent, Winthrop, M.D.

1866. Schaffer, Charles, M.D., Professor of Botany in the Pennsylvania Horticultural Society.

1870. Schell, Henry S., M.D., Surgeon to the Wills Eye Hospital.

1877. De Schweinitz, George E., M.D., Ophthalmic and Aural Surgeon to the Children's Hospital; Ophthalmologist to the Orthopædic Hospital; Assistant Surgeon to the Dispensary for Diseases of the Eye in the University of Pennsylvania.

1888. Seltzer, Charles M., M.D.

ELECTED

1875. SEYFERT, THEODORE H., M.D., Physician to the Gynecological Hospital and Infirmary for Diseases of Children.

1884. SHAFFNER, CHARLES, M.D.

1877. SHAKESPEARE, EDWARD O., M.D., Pathologist and Ophthalmologist to the Philadelphia Hospital.

1868. SHAPLEIGH, E. B., M.D.

1876. SHIPPEN, EDWARD, M.D., U. S. N.

1880. SIMES, J. H. C., M.D., Professor of Genito-Urinary and Venereal Diseases in the Philadelphia Polyclinic; Surgeon to the Episcopal Hospital and to St. Christopher's Hospital.

1873. SIMPSON, JAMES, M.D., Physician to St. Mary's Hospital.

1872. SINKLER, WHARTON, M.D., Physician to the Orthopædic Hospital and Infirmary for Nervous Diseases; Neurologist to the Philadelphia Hospital.

*1863. SMITH, A. K., M.D., U. S. A.

*1864. SMITH, EDWARD A., M.D.

*1856. SMITH, R. K., M.D.

1884. SMITH, ROBERT MEADE, M.D., Professor of Comparative Physiology in the University of Pennsylvania.

1875. STARR, LOUIS, M.D., Physician to the Children's Hospital; Clinical Professor of Diseases of Children in the University of Pennsylvania.

1884. STELWAGON, HENRY W., M.D., Instructor in Dermatology and Chief of the Dispensary for Diseases of the Skin of the University of Pennsylvania; Physician to the Philadelphia Dispensary for Skin Diseases; Dermatologist to the Northern Dispensary and to the Howard and Philadelphia Hospitals; Lecturer on Dermatology in the Woman's Medical College.

1888. STEWART, DAVID D., M.D., Physician to St. Christopher's Hospital.

1842. STILLÉ, ALFRED, M.D., LL.D., Emeritus Professor of the Theory and Practice of Medicine in the University of Pennsylvania; Consulting Physician to the Maternity and the Woman's Hospitals.

1846. STOCKER, ANTHONY E., M.D.

1889. STONE, J. FARRAR, M.D.

1871. STRAWBRIDGE, GEORGE, M.D., Clinical Professor of Diseases of the Ear in the Hospital of the University of Pennsylvania; Surgeon to Wills Eye Hospital; Ophthalmic Surgeon to the

ELECTED

Presbyterian Hospital; Surgeon to the Eye and Ear Department of the Philadelphia Dispensary.

1884. STRYKER, S. S., M.D., Obstetrician to the Philadelphia Hospital.

1886. TAYLOR, JOHN MADISON, M.D., Physician to Howard Hospital; Assistant Physician to the Orthopædic Hospital and Infirmary for Nervous Diseases; Physician to the Dispensary of the Children's Hospital.

1867. TAYLOR, R. R., M.D.

1887. TAYLOR, WILLIAM J., M.D.

1886. TAYLOR, WILLIAM L., M.D., Instructor in Clinical Gynecology in the University of Pennsylvania, and Chief of the Clinic and Assistant Gynecologist to the Hospital of the same; Surgeon-in-chief to the Beacon Service for Women.

1867. THOMAS, CHARLES H., M.D.

1869. THOMSON, WILLIAM, M.D., Honorary Professor of Ophthalmology in the Jefferson Medical College and Ophthalmic Surgeon to the Hospital of the same; Emeritus Surgeon to the Wills Eye Hospital.

*1854. TILDEN, W. P., M.D.

*1870. TURNER, A. PAUL, M.D.

1866. TYSON, JAMES, M.D., Professor of Clinical Medicine in the University of Pennsylvania; Physician to the Philadelphia Hospital.

*1852. TYSON, JAMES L., M.D.

1864. VANDYKE, E. B., M.D.

1873. VAN HARLINGEN, ARTHUR, M.D., Professor of Diseases of the Skin in the Philadelphia Polyclinic; Consulting Physician to the Philadelphia Dispensary for Skin Diseases; Dermatologist to the Howard Hospital; Lecturer on Dermatology in Jefferson Medical College.

1883. VINTON, CHARLES HARROD, M.D.

1885. WALKER, JAMES B., M.D., Professor of Practice of Medicine in the Woman's Medical College of Pennsylvania; Attending Physician to the Philadelphia Hospital; Lec-

ELECTED

turer on Clinical Medicine and Consulting Physician to the Woman's Hospital.

1886. WATSON, E. W., M.D.

1875. WEBB, WILLIAM H., M.D.

1883. WELCH, WILLIAM M., M.D., Physician to the Municipal Hospital for Contagious Diseases; Lecturer on Exanthemata and Vaccinia in the Medico-Chirurgical College.

1884. WHARTON, H. R., M.D., Surgeon to the Children's Hospital; Instructor in Clinical Surgery in the University of Pennsylvania and Assistant Surgeon to the Hospital of the same; Attending Physician to the Pennsylvania Institution for the Deaf and Dumb.

1883. WHELEN, ALFRED, M.D.

1878. WHITE, J. WILLIAM, M.D., Professor of Clinical Surgery in the University of Pennsylvania; Surgeon to the Philadelphia and the Maternity Hospitals.

1880. WILLARD, DEFOREST, M.D., Clinical Professor of Orthopædic Surgery in the University of Pennsylvania; Surgeon to the Presbyterian Hospital; Consulting Surgeon to the White and to the Colored Cripples' Home and to the Home for Incurables.

*1878. WILLIAMSON, JESSE, M.D.

1881. WILSON, H. AUGUSTUS, M.D., Professor of Orthopædic Surgery in the Philadelphia Polyclinic.

1874. WILSON, JAMES C, M.D., Lecturer on Renal Diseases in the Jefferson Medical College and Physician to the Hospital of the same; Physician to the Philadelphia Hospital.

1887. WILSON, JAMES F., M.D.

1884. WIRGMAN, CHARLES, M.D., Physician to the Hospital of the Jefferson Medical College and to the Howard Hospital.

1852. WISTER, OWEN JONES, M.D., Consulting Surgeon to the Germantown Hospital.

1865. WOOD, HORATIO C., M.D., Professor of Materia Medica, Pharmacy, and General Therapeutics in the University of Pennsylvania, and Clinical Professor of Diseases of the Nervous System in the Hospital of the same.

ELECTED

1880. WOODBURY, FRANK, M.D., Honorary Professor of Clinical Medicine in the Medico-Chirurgical College of Philadelphia and Physician to the Hospital of the same.

1866. WOODS, D. F., M.D., Physician to the Presbyterian Hospital.

1888. WOODWARD, CHARLES E., M.D., Physician to the Chester Co. Prison and West Chester Board of Health.

1878. WORMLEY, THEODORE G., M.D., LL.D., Professor of Chemistry in the University of Pennsylvania.

1860. WURTS, CHARLES STEWART, M.D.

1861. YARROW, THOMAS J., M.D.

1889. YOUNG, JAMES K., M.D., Instructor in Orthopædic Surgery and Assistant Demonstrator of Surgery in the University of Pennsylvania; Orthopædic Surgeon in the Out-patient Department of the Hospital.

*1840. ZANTZINGER, WILLIAM S., M.D.

1887. ZEIGLER, WALTER M. L., M.D., Assistant Aural Surgeon and Chief of the Dispensary for Diseases of the Ear in the Hospital of the University of Pennsylvania.

[It is particularly requested that any change of appointment, etc., may be communicated to the Committee of Publication before the first of November in each year, in order that the above list may be made as correct as possible.]

ASSOCIATE FELLOWS.

[Limited to Fifty, of whom Twenty may be Foreigners.]

ELECTED

1873. Acland, Henry W., M.D., F.R.S., Oxford, England.
1876. Barker, Fordyce, M.D., New York.
1877. Barnes, Robert, M.D., London, England.
1876. Bigelow, Henry J., M.D., Boston, Massachusetts.
1876. Billings, John S., M.D., U. S. A., Washington, D. C.
1876. Bowditch, Henry I., M.D., Boston, Massachusetts.
1886. Bowditch, Henry P., M.D., Boston, Massachusetts.
1865. Butcher, R. G., M.D., M.R.C.S., Dublin, Ireland.
1877. Byford, William H., M.D., Chicago, Illinois.
1877. Chaillé, Stanford E., M.D., New Orleans, Louisiana.
1886. Cheever, David W., M.D., Boston, Massachusetts.
1876. Comegys, C. G., M.D., Cincinnati, Ohio.
1876. Corson, Hiram, M.D., Norristown, Pennsylvania.
1876. Davis, N. S., M.D., Chicago, Illinois.
1876. Donaldson, F., M.D., Baltimore, Maryland.
1886. Draper, William H., M.D., New York.
1883. Fayrer, Sir Joseph, M.D., LL.D., F.R.S., London, England.
1876. Green, Traill, M.D., Easton, Pennsylvania.
1883. Heath, Christopher, F.R.C.S., London, England.
1874. Jackson, J. Hughlings, M.D., London, England.
1876. Johnson, George, M.D., F.R.S., London, England.
1876. Johnston, Christopher, M.D., Baltimore, Maryland.
1876. Jones, Joseph, M.D., New Orleans, Louisiana.
1876. King, James, M.D., Pittsburg, Pennsylvania.
1876. Kinloch, R. A., M.D., Charleston, South Carolina.

ELECTED

1877. Lister, Sir Joseph, Bart, M.D., LL.D.. F.R.S., London, England.
1865. Macleod, G. H. B., M.D., Glasgow, Scotland.
1886. McGuire, Hunter, M.D., Richmond, Virginia.
1876. Moore, E. M., M.D., Rochester, New York.
1876. Mowry, R. B., M.D., Allegheny City, Pennsylvania.
1873. Ogle, John W., M.D., London, England.
1874. Paget, Sir James, Bart., M.D., LL.D., F.R.S., D.C.L., London, England.
1876. Pollock, A. M., M.D., Pittsburg, Pennsylvania.
1876. Porcher, F. Peyre, M.D., Charleston, South Carolina.
1886. Reeve, John C., M.D., Dayton, Ohio.
1886. Senn, Nicholas, M.D., Milwaukee, Wisconsin.
1886. Shattuck, George C., M.D., Boston, Massachusetts.
1886. Thomas, T. Gaillard, M.D., New York.
1869. Valcourt, Th. de. M.D., Cannes, France.
1886. Whittaker, James T., M.D., Cincinnati, Ohio.
1886. Yandell, David W., M.D., Louisville, Kentucky.

CORRESPONDING MEMBERS.

ELECTED

1880. Carrow, Fleming, M.D., United States.
1880. Chiara, Domenico, M.D., Florence, Italy.
1886. Dey, Kanny Loll, M.D., Calcutta, India.
1889. Fedeli, Gregorio, M.D., Rome, Italy.
1885. Rendu, Jean, M.D., Lyons, France.
1886. Richards, Vincent, Goalunda, India.
1889. Strahan, John, M.D., Belfast, Ireland.

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REMARKS MADE BY THE PRESIDENT.

D. HAYES AGNEW, M.D.

[On assuming the Chair, February 6, 1889.]

Fellows of the College of Physicians: You have done me a very great honor in electing me to fill this position, altogether unexpected, altogether unsolicited, and I may say entirely undeserved. While deeply sensible of the honor you have conferred upon me, I may say, in assuming the duties, that I regret somewhat that I have been placed in this position, especially when I see around me many Fellows who, from long service and from their thorough knowledge of parliamentary usage and the art of managing scientific bodies, are better qualified to occupy this chair; but relying on your generous support, I shall endeavor to discharge the duties which devolve upon this position impartially and to the best of my ability.

ANNUAL ADDRESS OF THE PRESIDENT.

By D. HAYES AGNEW, M.D.

[Delivered December 4, 1889.]

Gentlemen: In obedience to a constitutional requirement, it becomes my duty at this time to deliver the customary annual address, setting forth the condition of the affairs of the College, and making such suggestions as may be deemed useful to our organization.

During the short time I have presided over the deliberations of your body I have been most forcibly impressed with the general decorum which has characterized the meetings, and also by the high order of work which has emanated from its Fellows. There have been twenty-four papers read before the College, all containing important original matter, and forming valuable contributions to medicine and surgery.

The Fellowship of the College has been increased by the election of ten new members, and two corresponding members, namely, Dr. Gregorius Feduli, of Rome, Italy, and Dr. John Strahan, of Belfast, Ireland. In sad contrast with this, the College has lost by death Dr. Caspar Wister, who died on December 20, 1888; Dr. E. T. Bruen, on March 31, 1889; Dr. Samuel W. Gross, on April 16, 1889; Dr. Elwood Wilson, on July 14, 1889, and Dr. Addinell Hewson, on September 11, 1889. During the same period one of our foreign associate Fellows, Dr. I. Milner Fothergill, of London, has paid the debt of nature, and also one of our foreign corresponding members, Dr. Adolphe Wasseige, of Belgium, on August 16, 1889.

The accessions to the Library of the College during the past year, from November 1, 1888, to November 1, 1889, by purchase and by exchange, amount to 2,066 volumes; 2,473 pamphlets, and 19,500 numbers of various medical journals. The entire number of volumes, including duplicates, and books on permanent deposit, is now 43,501. There are also on hand 13,862 loose pamphlets and reports. It would be desirable to have these classified by bringing together kindred subjects, and binding them into a permanent form.

The Directory for Nurses, established seven years ago, has proven to be an eminently successful branch of the College. I presume many of our Fellows are not aware of the extent to which the College is indebted, financially, to this department of its work. Over $1,540 has been contributed from this source to the money resources of the college; $1,300 of this amount has been expended in purchasing books for the library, and $240 has been paid to the hall committee for the various objects under its care. Since the founding of the Directory $7,269 has been realized, and of this amount the library has been benefited to the extent of $5,250; indeed it may be said, that the prosperity of our great library hangs upon the bounty of the Directory. These figures are so striking that I beg to urge upon the Fellows the importance of patronizing, on all proper occasions, this prolific source of our income. In this connection let me also say that I can see no sufficient reason why the clerical work of the College might not be simplified by turning over the funds derived from the Directory for Nurses to the general treasurer of the College.

During the past year the Museum of the College has been enriched by the addition of numerous specimens of great value, the most recent being those secured abroad by Prof. Joseph Leidy, during his late visit to the Continent; thus enlarging the field for anatomical and physiological study. It is gratifying to know that medical students from the different teaching bodies of the city are beginning to avail themselves of the advantages of the very large, varied and instructive collection which our museum contains. I am informed by the Curator that many of the most valued preparations in the larger room appropriated to the collection are in danger of being seriously damaged, in consequence of exposure to the light and heat of the

summer sun. Some steps should be early taken to correct this difficulty.

You will learn from the report of the Treasurer that, partly through the usual source of income and partly through the liberality of some of the Fellows, we close the year without a deficit. Such is a general *résumé* of the internal affairs of this body. There is no reason why the College of Physicians, composed as it is of men of recognized distinction in every department of medicine, should not become a controlling force in all matters which concern the public health. There is an immense field, one of almost limitless scope, which falls within the province of this College. Its power should be felt in the legislation, education, and social life of the people. Such papers as those which in 1887 were presented to the medical societies of the United States in regard to national maritime control, will rank in breadth of thought, circumstantial detail, and beneficent design, with any State papers which the country has produced on the subject of the exclusion of contagious disease. They give importance to the College and confer honor on the committee who framed them. Every year there should emanate from this body a paper, for general circulation, upon some topic connected with public health. Certainly in no city on this continent is there a wider or a riper field for such a work than in the city of Philadelphia. The food we eat, the water we drink, the buildings we construct, the minds we instruct, and the convicts whose liberty we restrain, are all replete with suggestive materials. It is to be hoped that the subject of tuberculous meat, which was brought prominently before us by a Fellow last winter, and which is attracting attention all over the civilized world, will not be allowed to slumber by the lukewarmness of the College.

In fine, the number of contributors among our Fellows to the science and art of medicine is exceptionally large in Philadelphia. Their labors are embodied in encyclopædic works, in works of the special departments of medicine, and in the current literature of our journals. Many of these men are busily occupied with professional cares. If it was possible to raise a moderate sum of money annually, as a compensation to some one whose duty it would be to make searches and collect statistics for the benefit of those engaged in

literary work, thus relieving writers of what is purely a mechanical labor, it would, in my judgment, prove a valuable service. In this way might the treasury of our great library be utilized to the greatest advantage. May the year on which we are about to enter be one of continued prosperity and usefulness for the College, conspicuous alike for the value of its work and the fraternal spirit inspiring the discussions of its Fellows.

NOTES ON THE TREATMENT OF PERITONITIS.

By JOHN H. MUSSER, M.D.,
PHYSICIAN TO THE PHILADELPHIA AND PRESBYTERIAN HOSPITALS.

[Read January 2, 1889.]

The following brief notes of some cases of peritonitis are given in order to call attention to the successful medicinal treatment of this disease in contradistinction to its surgical treatment. One cannot but look askance at the extraordinary surgical activity that prevails. It is possibly very well for the *experienced* abdominal surgeon to act in accordance with his convictions, particularly when in his skilful hands recovery takes place even if the operation has or has not been indicated. It is not well for him to *teach* that such measures must be resorted to in the larger proportion of cases. We are not all laparotomists, and hence a very large number of cases, particularly away from the surgical centres, must be treated by other means. The writer speaks by the card when he says the belief is very prevalent, particularly among the younger members of the profession, that surgical means alone avail. To attempt to restore confidence in medical means of treatment, to remove distrust, and to protest against one-sided lines of thought, is the object of these notes.

In addition, in order to show that the writer relies not on medical resources alone, and for their peculiar intrinsic interest, notes of a few cases which had to be operated on are included.

The patients herein classified occurred in the writer's private practice; the hospital cases and the cases seen in consultation are not recorded. Twenty-nine cases are noted: six males

under fifteen, four male adults; one girl, aged six, and eighteen female adults. Seven of the female adults were puerperal cases, of which three died. There were no other deaths. If the puerperal cases are excluded, twenty-two cases remain, with no deaths. This does not include the large number of cases of localized peritonitis or pelvic cellulitis which it falls to the lot of every practitioner to treat.

Puerperal Cases.—All may be considered of septic origin, although undoubtedly previous tubal disease (gonorrhœal) existed in three of the cases, two of which died, one in fact of these recovering from the peritoneal inflammation to succumb to pleuro-pneumonia.

Case I.—Primipara, æt. forty-two, contracted pelvis, craniotomy, furious general septic peritonitis. Recovery.

Case II.—Multipara, æt. thirty-four, septic peritonitis, fever, sweats, and local symptoms, extreme prostration. Recovery.

Case III.—Multipara (see below), æt. twenty-eight, septic peritonitis, enormous effusion, great hectic, and exhaustion. Recovery after aspiration.

Case IV.—Multipara (see below), æt. twenty-four. Recovery.

Case V.—Multipara, æt. thirty-eight, peritonitis, cause unknown. Throughout pregnancy premonitions of death, which took place on sixth day.

Case VI.—Xpara, æt. twenty-four. Prior to and during pregnancy symptoms of right tubal disease. Labor natural. Peritonitis, beginning in right iliac fossa on third day. Death on sixth day. Husband had gonorrhœa.

Case VII.—Xpara (see below), æt. thirty-seven. Labor natural. Long continued right side tubal disease. Husband dissolute. No history of gonorrhœa. Peritonitis began second day. Relief to all abdominal symptoms. Death, twelfth day, of double pleuro-pneumonia.

Of the four recoveries, a laparotomy saved one (Case IV.); of the three deaths, a laparotomy could possibly have saved one (Case VI.); there was no indication for abdominal section in one (Case V.); and neither medical nor surgical means could have saved the other (Case VII.). Indeed, the latter might justly be excluded or placed among the cured as far as peritonitis is concerned. After the recovery of the three, who were desperately ill, without abdominal section, one cannot but feel great confidence in nature's efforts, combined with judicious medical aid. The remaining cases were secondary to local abdominal disease

of three varieties: disease of the ovaries or tubes (eight cases); disease of the appendix (thirteen cases); and disease of the biliary passages (one case). Operative interference was resorted to in three instances; in two, for the primary acute attacks; and in one instance peritonitis ensued *after* a laparotomy for pyo-salpynx.

CASE VIII.—Female, æt. thirty-eight; married. Severe local pain, tenderness, and fever, etc., early recognized to be due to an ovarian cyst, which had rotated on its pedicle. Laparotomy by Dr. Parrish. Cyst almost gangrenous. Extensive peritonitis present. Recovery. (Reported by Dr. Parrish in the *Obstet. Soc. Trans.*, Philadelphia.)

CASE IX.—Female, æt. thirty-four; single. Appendicitis. Two attacks of local and general peritonitis. Subsequently tumor, and Parker operation by Dr. Porter in the Presbyterian Hospital. Recovery.

CASE X.—Male, æt. twenty-one. Appendicitis and peritonitis. Early recognition. Parker operation by Dr. Keen. Recovery. Of all the cases this one least merits to be classified among cases of general peritonitis. He was gravely ill, however.

CASE XI.—Here a pus-tube had been recognized by rational symptoms and physical signs. Female, æt. thirty-three. Dr. Price operated with his usual skill and care. A large pus-sac was opened, and possibly the peritoneum infected. Certainly a very severe peritonitis ensued, which was relieved by salines. The operation was not done to relieve peritonitis, while the inflammation was combated by medicinal means. The case, therefore, is placed under the non-operative list.

Nineteen cases remain, treated without operation. Eleven of these were secondary to appendix disease (six boys, one girl, two male adults, and two female adults); seven to disease in the pelvis, and one to inflammation of the biliary passages. Five of the cases of typhlitis were very grave; two had severe second attacks, and the temptation to resort to operative interference was great. Two of the pelvic cases were also very serious. Space will not permit detailing them.

SUMMARY.

Total cases	29
" deaths, including pleuro-pneumonia	3
No. of puerperal cases	7
" " deaths	3
" " non-puerperal cases	22
" " deaths	0

No. of times laparotomy or other surgical means employed:

In puerperal cases	1
" non-puerperal cases	3
Total	4
No. of deaths from laparotomy, etc.,	0
" " recoveries without operation—non-puerperal	19
" " " " " puerperal	3

In short, of twenty-two cases, three-fourths, certainly one-half of which were desperately ill, recovered from peritonitis, operative interference being necessary in three only.

Method of Treatment.—1. Local bloodletting until pain is very much relieved. Better too much than too little blood abstracted. Three to fifteen Swedish leeches, according to age. If in progress more than forty-eight hours, a large blister. Both of the above to be followed by hot applications, stimulating liniments, as turpentine stupes, if bleeding or blisters not used.

2. Liquid diet absolutely, stimulants administered freely to prevent collapse, to lessen its severity, and to prevent tympany. In severe cases whiskey and water alone, until the most urgent symptoms subside. Cracked ice, lime-water, champagne, iced, are used for thirst or vomiting, but their value is questionable; indeed, sips of hot water or hot whiskey and water are generally preferable. Freshly made koumiss has been of wonderful service, even after vomiting has set in.

3. Calomel in small doses, hourly, until the bowels are moved; then combined with opium in small amounts, to prevent diarrhœa, until ptyalism is secured. If the evacuations produce relief, as is often the case, the ptyalism need not be induced. One-eighth to one-half of a grain of the calomel is used, and has the advantage of being retained usually even if vomiting is obstinate, while it frequently allays this troublesome and weakening symptom. Inunction may be required for mercurialization.

4. If seen early, aconite to children and veratrum viride to adults is given until the pulse-rate is lessened. With veratrum

particularly a cool moist skin, a lower pulse-rate and fall of temperature, which should be brought about in twenty-four to forty-eight hours, are followed by relief to the local symptoms. Three to six drops of the veratrum (fluid extract) hourly usually suffice.

5. To relieve pain, morphia, hypodermically, in small doses, as infrequently as possible. It seems better to combine atropia with it for obvious reasons. Twice daily often suffices, with one-fourth, one-eighth, or one-twelfth of a grain. When an opiate is combined with calomel the morphia can be withdrawn generally. If morphia disagrees, as one often finds it to do, codeia will often suffice, given by the methods suggested by Brunton.

6. If the bowels are not opened in forty-eight hours by the calomel, if the case is not one of strangulated hernia or acute obstruction of the bowel by a twist or by adhesion by a band, an enema of warm sweet-oil (Oj), and then of tepid water are used with extreme care. It is undoubtedly of service to have the bowels moved as soon as possible. That peristalsis does not do as much harm as is usually taught we know, because diarrhœa attends some cases of peritonitis, as we have seen more particularly in peri-cæcal abscess. Moreover, accumulations of gas are prevented, a thing highly desired. Indeed, it may be put down as a cardinal rule in therapeutics that in all affections of or about the tubes or channels, a patulous state of the canal must be obtained if possible.

7. There is no doubt, even when on the verge of collapse, that life can be saved by persistent and active stimulating medication. This must usually be by subcutaneous injection. Whiskey, digitalis, atropia, and amyl nitrite all have been of service.

INDICATIONS FOR OPERATIVE INTERFERENCE.

The writer seems justified in presenting the following suggestive conditions for operative interference from a study of hospital and private cases, and cases seen in consultation. They

may not be complete, but they have had practical exemplification in his hands:

1. Cases of fulminating peritonitis. Rapid advances of all symptoms, and particularly of excessive tympany, vomiting, rapid, feeble pulse, and undue restlessness. Operative measures are urgent, especially if it is the second attack, or if a primary local peritonitis has occurred beforehand.

2. Cases in which collapse is growing imminent in spite of treatment. The peculiar vomiting and a rapid pulse, growing feebler, with low temperature, are suggestive.

3. Cases in which pus is present in the peritoneal cavity or adjacent to it, as indicated by

(*a*). The presence of a tumor, presumably containing pus, either on external examination or by rectal or vaginal examination. Œdema over a tumor is important.

(*b*). The general phenomena indicative of retained pus. The classical symptoms of hectic need not be present. Night-sweating alone may be a sufficient or markedly suggestive indication of hectic.

4. Cases with grave symptoms, the antecedent history or the early symptoms of which point to retention or septic inflammation of the appendix, the biliary passages, or of the tubes. Previous gall-stones, gonorrhœa in the female, typhoid fever, appendicitis, or intestinal perforation, are weighty matters in favor of an operation. Ulcer of the stomach or bowels belongs to this class.

5. Peritonitis, due to intestinal obstruction, as by hernia, twists, intussusception, etc., not relieved in twenty-four hours, requires laparotomy by all means.

6. There is, in cases that are fatal, a general appearance of the patient and a mental state often of restless apathy, which is so hard to describe, but when once seen is never forgotten, which intuitively leads one to prognosticate death. These cases should be given the benefit of every measure; and certainly in one instance a laparotomy, under these circumstances, saved life.

7. Age is an important factor. A young person or child can be more ill and recover from a peritonitis more rapidly than an

old one. An operation at this age can be done later in the case, with more hopes of success, than in an adult over forty.

8. Are there any symptoms, the presence or absence of which would help one to arrive at any conclusion? The rapid, thready pulse, the painless watery outpouring from the stomach, the apparent indifference to the gravity of his condition, even when in collapse, with unclouded intellect, which the patient has; the presence of pus, causing threatening symptoms, have been referred to. The absence of pain after the first forty-eight or seventy-two hours, either on movement or pressure; the absence of tympany, and the suppression of the urine are grave phenomena. Instead of distention, the abdomen is sometimes flat, and on palpation the walls are rigid and present a board-like hardness. In two obscure cases this rigidity of the muscles was the strongest evidence of acute suppurative peritonitis, which the autopsy unhappily proved to be present. Without these phenomena the disease could not have been positively recognized, and in fact its presence was disputed by the colleagues in consultation.

Sometimes we are morally certain of the presence of pus, as shown by mild hectic, and yet we cannot localize it. If the general symptoms are not alarming we can wait. One of the above patients was not operated on until five months after the first attack of peritonitis had subsided. Throughout this time irregular chills, fever, and sweats occurred; and pain in the right inguinal region led to the recognition of a slowly developing tumor, which Dr. Porter successfully opened.

Some of the above cases presented some features of interest, which will be briefly detailed.

1. (Case III. above.)—The writer was called on tenth day after patient's confinement and the seventh of her illness. Much prostrated, extremely anæmic, pulse thready, sweats profuse, temperature high, general abdominal pain and tenderness. Abdomen distended very much by fluid. Laparotomy advised, but absolutely refused. After five days of futile treatment, save success in checking the flagging strength by stimulants, the abdomen was tapped with the ordinary trocar and canula, exploratory tapping having been done before with a hypodermic syringe. The fluid was thus removed in this unscientific way, because it seemed impossible to the

writer that absorption of the large amount could take place, and hence it was the only chance, short of abdominal section, to save the life of the patient. A large wooden pailful and a half of a bloody purulent fluid was removed, my friend, Dr. Wm. Gross, assisting. Fifteen days afterward the patient was about the room, but for a long time the abdomen was extremely tender, and not until ten days after the operation did hectic cease.

One almost marvels, considering the character of the effusion, at the cure. Such it was, however, and while not brilliant, it was complete. By the trocar not all of the fluid could have been withdrawn, and hence, to nature's means of curing these cases, much was left. The eminent interest of the case, therefore, is in the fact that cases of peritonitis, with purulent effusion, will get well even under the most unfavorable circumstances. The poor woman had not the advantages of good nursing, good hygienic surroundings, and good food. Pluck and good whiskey alone favored her. This case is recorded more particularly to show that we can probably oftener withhold the knife than we do, and also to give courage to those whose patients cannot have the presence of the skilled surgeon. It is not to advocate such a plan of treatment.

2. (Case IV. above.)—Female, æt. twenty-four. Natural labor completed before the arrival of the physician. On the second day local pain, gradually increasing, with the formation of a tumor in the region of the right ovary. Great hectic and exhaustion. Ten days after confinement laparotomy by Dr. Price. Recovery.

This is an illustration of the advantage of surgical aid and of the value of operating even in the early puerperium.

ARTHRECTOMY OF THE KNEE-JOINT AS A SUBSTITUTE FOR ARTICULAR EXCISION;

With the Exhibition of a Patient.

By JOHN ASHHURST, Jr., M.D.

[Verbal communication made February 6, 1889.]

The case brought before you to-night is of some interest, being illustrative of an operation which is attracting a good deal of attention among surgeons, particularly those who have to do with diseases of childhood. This is a comparatively new operation, having been first suggested by Wright, of Manchester, some seven years ago. His first case was recorded in 1881. He has operated in a certain number of cases since then, but it has been within a very short time, hardly over a year, that the procedure has been taken up by other surgeons. It is now practised by Owen and Parker, of London, and by others. It has been adopted in Germany and in France. In France the sentiment is rather against the operation. The opinion of the majority of French surgeons who have employed this substitute for excision is, that it is equally as dangerous, and the result no better, and in some respects not as perfect. In Germany the sentiment is more favorable to arthrectomy, and in England it is still more so.

My experience with this operation is very limited, as I have employed it only twice. The second patient is still under treatment, and the case which I record to-night is, therefore, the only completed one which I have to report. I think that, as far as a single case can, it shows that the operation is a

good one, and that it has a distinct field of usefulness in cases where the surgeon hardly feels justified in resorting to excision. The history of this case is as follows:

The boy is twelve years of age, with a good family history. The father is living and healthy, the mother living and phthisical; but whether or not there is a tuberculous taint inherited from the mother is uncertain. The belief of the family is that the mother's health was broken down by nursing this boy, and that the phthisis was developed after his birth. The lad was under my care, in the Children's Hospital, more than seven years ago, when five years of age, for the same condition of the knee-joint. At that time I divided the hamstring tendons and applied a splint, and he left the hospital. This is a practice that I have followed for a number of years in young children, for excision is not a very successful operation in the earliest years of life. In this way a serious operation can often be postponed for some time. The patient returned to the hospital this fall, having in the meantime been under the care of the surgeons in the out-department. His knee-joint, while not getting much worse, did not get better. It was one of those cases which seem too bad to be let alone, with no prospect of benefit from conservative measures, and, on the other hand, not bad enough to justify any very dangerous operation. This seemed to be a case in which it might be proper to do this comparatively new operation of arthrectomy.

The term arthrectomy might properly be applied to the operation of excision itself, but it is understood as applying to the removal of all of the soft tissues, without disturbance of the bones. Some surgeons have employed a longitudinal incision on one or both sides, passing behind the patella and scraping away the soft tissues. Others make the incision across the patella, saw through that bone, clear out the joint, and bring the two segments of the patella together with stitches. The plan which I have adopted, and which is, I think, preferable, is to make the incision just as for excision, below the patella, through the ligament, turning up the patella and getting full exposure of the joint. Then with knife, scissors, and curette, I carefully remove all morbid tissues that can be dissected out, the synovial sac, semilunar cartilages, and any portions of the articular cartilages and bones that may be diseased. The greater portion of the synovial membrane can be dissected out with the knife, scissors, and forceps. The portion at the back of the joint cannot well be

thus dealt with, but can be removed with the curette. The articular cartilages should be thoroughly scraped. If any patches of carious bone are found, they should be removed. A drainage tube is then inserted, the wound closed, and the limb placed on a posterior splint, just as after excision.

This boy was operated on October 3, 1888. Here is his temperature record, which was kept for two months. It shows that there was very little reaction from the operation. The highest point reached was 100.6°, and that upon only one occasion. On five occasions it reached 100°. The result of the operation is an almost perfectly straight leg, with firm union between the articular cartilages. Some surgeons have attempted to obtain motion, but I think it better to be satisfied with fibrous anchylosis. This is simply imitating Nature's mode of effecting a cure in comparatively mild cases, where fibrous anchylosis takes place. In the same way, excision is an imitation of Nature's method of effecting a cure in bad cases—that is, by bony union.

The advantages of this operation over excision are, in the first place, that the length of the limb is not interfered with, and that there is no risk of consecutive shortening. One of the most serious prospective risks after excision is that, from implication of the epiphyseal cartilage, the growth of the limb may be prevented. Most surgeons are familiar with the case of Mr. Pemberton, of Birmingham, where the shortening amounted to eight or ten inches. This is, of course, unusual, but there is risk that the limb will not grow in proportion to the rest of the body. As this operation does not involve the epiphyseal cartilage, there is no risk of consecutive shortening. In the second place, the shape of the limb is preserved. This is, however, a point of minor importance. In the third place, there is no risk of that inward turning of the lower portion of the limb, so common after excision. Surgeons who have done many excisions are familiar with the fact that it is sometimes almost impossible to prevent a certain amount of axial deformity, the lower part of the limb turning in while the upper part rolls out. In arthrectomy the condyles are preserved and the head of the tibia fitting between them prevents the occurrence of this deformity.

An objection to the operation urged by French writers is,

that there is apt to be consecutive flexion. This also occurs after excision, and can be avoided by maintaining a firm support as long as may be necessary. I think that in a case like the present it would be better to perform arthrectomy and then retain a plaster bandage or a leather support for even a long time, than it would be to perform excision.

I, of course, do not suppose that this operation will ever take the place of excision, for the majority of cases in which operative procedures are called for are so far advanced that excision is the only remedy; but there are cases in which the disease is limited to the soft parts, and here this operation has its legitimate field. In this case the boy had been under observation for eight years, and the condition had finally become so bad that it seemed necessary that something should be done. There are then a certain number of cases in which this operation may properly be performed, coming in between the simple rectification of the deformity by tenotomy and the graver operation of excision.

It is about four months since this limb was operated on. It is now strong, and the boy can walk on level ground without crutches. He does not require a high shoe. Before the patient goes home, a plaster bandage will be applied, and he will wear this for a long while, reporting from time to time, so that the occurrence of consecutive flexion may be avoided.

A CASE OF POTT'S DISEASE FROM INJURY, FOLLOWED BY COMPLETE PARAPLEGIA, TREATED SUCCESSFULLY BY SUSPENSION.

Exhibition of Patient.

By WHARTON SINKLER, M.D.

[Read February 6, 1889.]

History of Patient.—John T., aged eighteen, native of Ohio, was admitted to the Orthopedic Hospital and Infirmary for Nervous Diseases, February 16, 1888. Father and mother both living and healthy. He is the eldest of three children; others are healthy; there is no history of consumption in the family; has never had any of the diseases of childhood; always healthy and strong.

In September, 1886, some of his school-fellows "bumped" him against a tree. This was done by four boys, each taking a leg or arm and bringing the buttocks forcibly against the tree. Acute pain in the spine was produced by this at the time, and afterward, in either sitting or standing, pain was remarked; but when lying on his back he was comparatively easy and free from pain. The pain was first greatest in the epigastrium and radiated thence into the back, down into the legs, and up into the shoulders. About three weeks after the injury, he accidentally discovered a lump in the dorsal region of the spine. Soon after observing this, the lump became very painful and was sensitive to the touch. He continued to sit up and walk, but progressively became weaker, and after the second week in January, 1887, was unable to walk at all.

About three months after the injury, he began to lose sensation in the legs; the knees began to get stiff in the spring of 1887. He has been unable to sit up, but was comparatively comfortable in bed.

Condition on Admission.—A tall, slim boy; weight eighty-four pounds; angular curvature at seventh dorsal vertebra; can sit up with the aid of a leather corset which he has worn for some months. He is unable to move

the legs at all; knees and ankles generally in a state of rigidity; knee-jerk grossly exaggerated; patellar clonus present; ankle clonus very marked on both sides; toe reflex present—(this is very striking on both sides). The condition known as *spinal epilepsy* readily induced; plantar reflex exaggerated; no cremasteric or abdominal reflex can be elicited; sensation is impaired, but is not lost. He can localize and distinguish touch anywhere on legs or feet; no interference with the functions of bladder or rectum. There is no pain in the spine.

There is a bed-sore the size of a ten-cent piece over the sacrum. The circulation in the legs is poor; the feet are constantly cold. His temperature is above normal, morning and evening; varying from 100° to 102° F. in the evening; in the morning, 99° to 100° F.

He was ordered massage, and the cautery was used on either side of the angular curvature. He was also ordered cod-liver oil and milk-punch.

A plaster jacket was applied soon after his admission, but as he fainted before the plaster could set, it had to be removed, and in two days a second jacket was applied. This could only be borne for three or four days, on account of interference with respiration; and it was removed on the second day. A third jacket was applied at the end of another week, and in this he was comfortable.

The bed-sore, however, continued to increase in size and depth.

On March 18th he was suspended by means of a padded leather chin and occipital piece from a bar of iron projecting over the head of his bed. This was placed high enough above the bed to allow him to sit upright upon the bed. He was able to bear the extension for nearly five minutes on the first day. On each subsequent day the time was extended, until he was able to bear the extension for half an hour twice a day.

On April 17th, the plaster jacket having been removed on March 29th, ice and hot water were applied to the spine over the region of the curvature, for ten minutes four times each day—the ice and hot water being applied alternately; first the cold, followed by the heat.

May 14, he was placed in a chair, and extension made from a rod attached to the back of the chair and bent over the head.

About this time he began to have slight power of motion of the toes; this was followed by power to flex the feet and to flex the thighs.

June 25, fluid extract of ergot, twenty drops three times a day, was added to the treatment; and this was continued until July 31st.

July 23, the following note was made: Condition much improved; complains of no pain; can move legs, drawing up the knees lying on his back in bed; sensation is improved, but is not perfect; clonus is less, but still marked; knee-jerk still exaggerated; toe reflex still present.

The application of heat and cold to the spine was continued until October 18th.

In August he was able to lift the feet from the floor as he sat on the

chair. One of Kolbe's spinal apparatuses was applied on July 31st, and he has continued to wear this when sitting up ever since; it is removed when he is in bed.

In the middle of August he could walk a few steps in the wheel crutch.

In October he began to walk about with crutches. He has continued progressively gaining strength in the legs, and is now able to walk about at pleasure, but has not yet been allowed to dispense with his crutches, although quite strong enough to do so.

His weight is now ninety-seven pounds. There is no tenderness over the curvature, but there is no material change in its shape. The reflexes are all still exaggerated, but to a moderate extent; and the tremor of the legs is no longer excited by moving them vigorously.

SPINAL LOCALIZATION IN ITS PRACTICAL RELATIONS.

By CHARLES K. MILLS, M.D.,
PROFESSOR OF DISEASES OF THE MIND AND NERVOUS SYSTEM IN THE PHILADELPHIA POLYCLINIC, ETC.

[Read March 3, 1889.]

Part I.

GENERAL CONSIDERATIONS.

Spinal localization, particularly in its practical relations to surgical procedure, is one of the topics of the hour which has not yet been fully discussed in any of our Philadelphia medical societies. Although its proportions do not approach those of cerebral localization, the subject has sufficient literature and interest to allow of lengthy treatment and open a wide field of discussion; but it will be necessary for me to be brief in order to cover the ground.

Spinal trephining is by no means a recent operation; it has an ancient history both as regards suggestion and actual performance. Horsley[1] speaks of it as known to surgery since the time of Heister (1757), but Lidell attributes the suggestion to Paulus Ægineta. An operation of M. Louis, performed in 1762, is sometimes referred to as the first instance of spinal resection, but, as stated by Ashhurst and Lidell,[2] it was not a resection at all, but merely an extracting of some loose fragments of bone on the fifth day in a case of gunshot fracture

[1] Med.-Chir. Trans., vol. lxxi., Lond., 1888, p. 383.
[2] Intern. Encycl. Surg., vol. iv.

involving a dorsal vertebra, an operation which was performed twenty-four times during our civil war with satisfactory results. Genuine spinal trephining was first performed by Henry Cline, June 16, 1814, for fracture of the twelfth dorsal vertebra. Horsley, writing last year, cites Erichsen as stating that it has been performed in all about thirty times; but Ashhurst, about the same time or earlier, tabulated fifty cases. Of these fifty cases, thirty-three died, seven were not benefited, four were relieved, two recovered, and in four the result was unknown. I have collected fifteen cases in addition to those in Dr. Ashhurst's table, and doubtless others have escaped me. In addition to these I have personal knowledge of three cases in which spinal trephining has been performed, but in which the accounts of the operations have not yet been published.

While we have here, however, a record of sixty-eight cases in which the operation of spinal trephining has been performed, a close study of the details of these cases shows that very few operations were guided strictly by the rules and principles of spinal localization. External pointers, as deformity, depression, or swelling, were commonly present. Gowers' and Horsley's case of spinal tumor is perhaps the most brilliant example of localization, and vies with the cases of Macewen and Abbe in brilliancy of result.

Many experimental investigations of the functions of the spinal cord have been made by Brown-Séquard and a host of physiologists, but the first experiments to determine the exact functions of the horns of the cord at various levels were these of Ferrier and Yeo,[1] in 1880. Pathological observations had been made prior to this time and have been since, and are largely confirmatory of the results obtained by these investigators. In 1884, Starr[2] collected most of the work on this subject up to that date. Ross[3] has worked in the field of sensory localization to good purpose, contributing an excellent

[1] Proc. Royal Soc., No. 212, and Brain, July, 1881, vol. iv. p. 223.
[2] Amer. Journ. Neur. and Psych., vols. ii. and iii. p. 443.
[3] Brain, January, 1888.

paper on the "Segmental Distribution of Sensory Disorders." Much material of a highly practical character will be found embodied in Gowers' *Manual of Diseases of the Nervous System* and in Seguin's contributions to localization in the *American System of Practical Medicine.* Recent valuable papers, partly neurological and partly surgical, on spinal localization are those of Thorburn.[1] This writer records nineteen cases of injury to the cervical region of the spinal cord, studied with the view of determining the exact spinal localization as indicated by such injuries. He has also contributed a valuable article on injuries of the cauda equina. These contributions will be made use of later in this paper. Osler[2] has published a brief paper on lesions of the conus medullaris and cauda equina, and on the situation of the ano-vesical centre in man. Other papers on allied topics have been contributed by Bernhardt, Oppenheim, and Kirchoff.

It is important to study closely every case of supposed spinal focal lesion, whether from injury or disease. Reports of such cases, particularly when accompanied by autopsies, will continue to be useful until a number of points now obscure become clear; but even without autopsies they may be of great value if minute and accurate. Some of the papers on cerebral localization, most valuable because of their suggestiveness, have been purely clinical and largely speculative; as, for example, some of Hughlings Jackson's and Broadbent's on affections of speech. One of the most instructive articles on spinal localization, that of Remak[3] on the localization of the lesion in anterior poliomyelitis, is based entirely on clinical cases. From a careful study of many cases, Remak shows how various groups of muscles are frequently affected together, and, reasoning back to the lesion, concludes that disease in a certain segment or adjoining segments of the spinal cord will produce paralysis in a group of muscles which act together. He made some highly interesting observations with reference

[1] Brain, January, 1887; January, 1888; and October, 1888.

[2] Med. News, vol. liii., December 15, 1888.

[3] Arch. f. Psych., ix.

to palsies of particular muscles or muscular groups. These investigations will be referred to again.

NOTES OF CASES OF SPINAL FOCAL LESIONS, WITH AUTOPSIES.

Let me first refer briefly to a few cases of spinal focal lesion, with autopsies, occurring in my own experience, briefly commenting on them with reference to the possibility of successful operation.

While it has been my fortune to see a large number of patients suffering from intraspinal focal lesions, I have made comparatively few autopsies on such cases. I find among my notes, however, records of six post-mortem examinations, three of which have been reported. Of the six cases, two and perhaps three might have been trephined with some show of success. One of these was an injury in the lower dorsal region in which the lamina of the twelfth dorsal vertebra had been broken and driven inward, a case similar to one of the operative cases reported by Macewen—similar even to the vertebra injured. A bony mass was present about half an inch long by one-quarter of an inch wide in the external layer of the dura, just at the lower end of the spinal cord. This mass, which was like the bony concretions sometimes found in the falx cerebri, could have been removed by operation, after elevation of the depressed and broken spinal arch.

In another case operation might have been of benefit if performed immediately after the accident. The most important features of this and the first case were as follows: A history of severe injury to the middle and lower spine; this was followed at once by pains in the back and limbs, spinal rigidity, anæsthesia and paralysis of the lower limbs and of the bladder and bowels. The condition remaining was one of motor paralysis, wasting, contracture, depressed electro-contractility, coldness and lividity, peculiar zones of anæsthesia, and abolition of the tendon reflexes in anæsthetic regions. In this second case, at the upper edge of the twelfth dorsal vertebra adhesions were found which continued down the canal

for three or more inches. At the commencement of the cauda equina, which was about the centre of the adhesion, caries of the first lumbar vertebra was found, the portion involved being the inner surface of the laminæ close to the junction of the body. Above and below the caries the adhesions were most persistent. The conus medullaris, cauda equina, and membranes were a glued mass. For an inch or two more above and below this the membrane was considerably thickened, a deposit being present of a calcareous nature.

In a case of caries and dural tumor, of which I exhibit specimens, the patient might perhaps have been benefited by relief of pressure. This patient, a man seventy-two years old, was admitted to the Philadelphia Hospital with a history that he had been lame in his left leg, the result of a fall, since he was ten years old. No relatives or friends could be found, and, as his mind seemed somewhat clouded, it was difficult to obtain from him any statement of value as to his past condition. It was learned, however, that three or four months before admission he had an attack in which he suffered from diarrhœa and fever; that he became weak in both legs, but largely recovered power in them, until four weeks before admission, when he became a complete paraplegic.

He had no paralysis of the face and arms. His left leg was shorter than his right, and was considerably atrophied, measurements showing about an inch difference between the limbs. Motor paralysis was complete in both legs, but he had control over the bowels and bladder. Tactile sensation was diminished in both legs, and on his trunk to a line on a level with the third dorsal spine behind and the fourth rib in front. Pain and temperature sense were preserved. He complained of a belt-like feeling around the waist at the highest level of the anæsthesia. He had a sacro-ischiatic bedsore nearly central in position. He died a few days after admission to the hospital. The brain showed no gross lesion. On opening the spinal canal, when the line of the fourth dorsal vertebra was reached the dura mater was found to be strongly adherent, requiring dissection, and a small tumor or enlargement was

attached outside of this membrane on the lateral aspect of the cord. It was about three-quarters of an inch in length and one-third of an inch in thickness. Possibly the cord was slightly diminished in general bulk from compression at this point, but it was not abnormal in consistency, and no hemorrhage or inflammation was present. The body of the fourth dorsal vertebra throughout was found to be eroded and in a carious condition. For a little distance below, the periosteum was detached and the bone partly exposed. The disease of the bone had destroyed nearly the whole of the interior of the body of the vertebra. Below, the index finger could be passed into the cavity, while above was a shell or bridge of bone. The vertebra was not displaced. The intervertebral cartilage between the fourth and fifth dorsal vertebræ was also necrosed. The upper lobe of the right lung was strongly adherent to the bodies of the third, fourth, and fifth dorsal vertebræ. A communication existed between the space in the decayed vertebra and the pleural sac.

In two cases of syphilomata of the dura mater of the cervical region, operation would have been useless from the nature, extent, and high level of the lesions.

The sixth case was one of hemorrhage into the cauda equina, the symptoms being similar to those in the second case referred to above. The question whether in such a case early trephining at the lowest point of the lumbar region would have been feasible, and would have offered any prospect of relief, will be considered later.

SPINAL OPERATIONS GUIDED BY LOCALIZATION; SPINAL FRACTURES AND DISLOCATIONS.

Trephining the spine in whole or in part, guided by the rules of localization, may be resorted to for fracture, dislocation, tumor (including certain hypertrophic inflammatory pachymeningeal lesions), hemorrhage, abscess, caries, and neuralgia.

Page[1] has said, with reference to fracture and dislocation, that trephining of the spinal column can be of no advantage; but this is certainly far from being true of the former, and even in some cases of dislocation the operation may be of assistance to the surgeon in bringing about reduction. Other eminent surgeons, as Lidell and Ashhurst, hold that more lives are lost than saved by interference. Whether operation in cases of fracture and dislocation will or will not succeed is largely a question of exact diagnosis of the extent and location of the injuries. When, for instance, it can be demonstrated by close study that the brunt of the injury has been borne by the spines and laminæ, operation should be at once performed, and even when in doubt it is better to explore rather than to hesitate in the face of paralysis or death.

An interesting recent successful case is one performed in 1887 by an English surgeon, Mr. Jones, reported by Thorburn in *Brain* for January, 1888, and also later, as to result, in the *British Medical Journal* of October 20, 1888.

The patient, a collier, aged fifteen, was injured by a heavy stone striking him on the shoulders and back. He received a fracture of his right femur, and was also instantly rendered paraplegic. The limbs presented a position of partial paralysis, with wasting, lost faradic contractility, absent knee-jerk, changed skin reflexes, paralysis of the bladder, cystitis, and priapism. Sensation was blunted or lost over special nerve areas in the lower limbs. The third lumbar spine presented a prominence. To the left and above was another bony prominence. Trephining was performed. The detached spine of the second lumbar vertebra was removed, and showed a gap between the arches of the second and third, filled with dense cicatricial tissue. By means of bone-forceps the arch of the second lumbar was now almost entirely removed, exposing the membranes of the cord, which had obviously been compressed by it. Around these membranes was also cicatricial tissue, which was not interfered with. Twenty months after the operation the patient was exhibited at the Manchester Medical Society by Mr. Jones. His condition was then very satisfactory. Sensation was normal in both legs, which were, however, much wasted, and there was evidence of degeneration in the muscles of the left limb. The movements of the knee-joint were normal, but those of the ankle were limited to extension, flexion being practically abolished. He had perfect control over his

[1] Heath's Directory of Surgery, p. 134, quoted by Horsley.

bladder, and could urinate normally. He was able to walk three miles with short intervals for rest. When standing he was obliged to lean on some support unless he bent his knees. He was gradually improving.

An equally interesting, but unsuccessful, case, operated upon by Mr. Hardie, is also reported by Thorburn.[1]

The patient was a man thirty-eight years old. The operation was performed October 26, 1887. The patient had a fall of six feet from a wagon, and immediate paralysis without loss of consciousness followed. He had pain across the shoulders and shooting down the arms to the elbows, and arching backward of the cervical vertebræ. The lower limbs, abdomen, and thorax were absolutely paralyzed. His respiration was diaphragmatic, with sense of dyspnœa and cough. All the muscles of the right upper extremity were paralyzed, and the extrinsic muscles of the right upper limb also paralyzed,—the pectorals, spinati, latissimus. The neck muscles were normal on both sides. The muscles of the left upper extremity escaping paralysis were the biceps, supinator longus, deltoid, supra-spinatus, and infra-spinatus. Both palpebral fissures and pupils were smaller than usual. Anæsthesia extended as high as the third rib in front and the sixth cervical spine behind; on the right side there was sensation over the region of the deltoid and down the outer side of the limb to the thumb. No hyperæsthesia was present. Knee-jerk, and plantar, cremasteric, gluteal, and epigastric reflexes were absent. The temperature was normal. The urine was retained, the penis turgid. His pulse was sixty-six, with a marked respiratory wave. A vertical incision about four inches long was made over the cervical spines, with its centre opposite the fifth. The muscles being cleared from the laminæ, an interval of about a quarter of an inch was found between the fifth and sixth spines, and the fifth vertebræ appeared to be slightly displaced forward. The laminæ of the fifth and sixth vertebræ were removed by bone forceps and the dura mater exposed, presenting a perfectly normal appearance without any trace of hemorrhage. The patient died the next day. The heart-beats, which were very feeble and infrequent, continued for about ten minutes after respiration had ceased, but stopped at 3.35. The disk between the fifth and sixth cervical vertebræ was found to be ruptured, the former bone projecting very slightly forward. No fracture was discovered. The dura mater was uninjured, but the cord was flattened opposite the seat of the injury, and was much contused for about an inch above and below, containing hemorrhage in its substance and in the central canal; elsewhere its structure was normal.

In lesions of the cauda equina which cause compression, whether fracture, hemorrhage, or abscess, but particularly

[1] Brain, October, 1888.

fracture, trephining is undoubtedly indicated in most cases, and in some, with our present light, it is imperatively demanded. This view I take from personal experience of cases with autopsies, and of others which I have studied clinically. The same view is strongly taken by Thorburn,[1] who shows that some of the objections which are valid against trephining higher in the vertebral column, do not apply so fully to injuries of the cauda equina. He advocates trephining in injuries of the cauda equina, always bearing in mind the following conditions:

"1. We must be sure of the localization.

"2. We should, if possible, wait for a reasonable period, say six weeks, before operating, and should then do so only if the patient shows no signs of spontaneous recovery.

"3. Should the paralysis of the bladder be early followed by severe cystitis, and should we suspect secondary renal troubles, what are we to do? On the one hand, not to operate probably means death; and on the other, the risk of operation is enormously increased. Further experience may enable us to settle this problem, but at present the outlook appears to be sufficiently gloomy. A third course has suggested itself to me, but I have had no opportunity to test its results,—viz., to drain the bladder by means of a suprapubic cystotomy. The object of draining the bladder is obvious, and the suprapubic appears preferable to the perineal route, because we are thereby enabled to construct our fistula through parts which are not anæsthetic and predisposed to slough. If we could thus prolong life for a time, we might afterward proceed to operate upon the spinal column with fair hopes of a successful result, and encouraged by the great improvement effected in the case of R. M. C., and the cure obtained in a case recently published by Lauenstein, in which there was complete paralysis of the lower limbs, etc., from a dislocation of the twelfth dorsal vertebra, and where a perfect cure resulted from the removal of the misplaced arches. It would appear, therefore, that whether 'trephining the spine' is or is not justifiable in cases of injury of the cord, it is certainly the proper treatment to pursue in those of the cauda equina, and hence the importance in such cases of making an exact diagnosis."

SPINAL TUMORS.

In most cases of tumor of the spinal cord or its envelopes, operation would not for several reasons be completely success-

[1] Brain, January, 1888, p. 406.

ful. It would, however, be entirely successful in some, and I have come to the conclusion that almost all cases should be trephined for the chance of success, or to relieve pain and diminish the effects of pressure. When the growths spring from the bone or dura mater the chances are, of course, the best.

Even if the neoplasm is situated in front, or surrounds a large part of the cord, trephining might be successful, for during operation the lateral and even the anterior aspects of the cord can be explored, the latter certainly by cutting one or two spinal nerves. This can be seen by examining the cord in position in a fresh specimen. With a little care, using a blunt hook, the cord can be rolled and lifted so as to examine it on almost every side in position. Erosion or caries, with perforation of the body of the vertebræ, can also be determined by careful explorations with properly curved probes thrust around and beneath the exposed cord.

Three operations for spinal tumor, all of them successful, have been reported by Macewen,[1] Gowers and Horsley,[2] and Abbe.[3]

The case of Macewen was a boy, nine years of age. The operation was performed May 9, 1883. The main symptoms were complete sensory and motor paraplegia, with incontinence of urine and feces. Angular curvature was present, most marked between the fifth and seventh dorsal vertebræ. The laminæ of the fifth, sixth, and seventh dorsal vertebræ were removed. A fibrous neoplasm attached to the theca, between it and the bone, was dissected off. After this, cord pulsations, which had been absent, returned. Twenty-four hours after the removal of the pressure the limbs had improved in color and were distinctly warmer; the spastic rigidity had greatly lessened. The sense of tickling had returned to the soles, and that of touch had improved. After eight days the first return of movement was observed; soon after he had perfect control over his sphincters. Six months subsequently he was able to go about without support, and five years afterward he walked three miles to pay the doctor a visit. He attended school regularly, joined in all the games, including foot-ball, and said he felt quite strong. Charcot

[1] Lancet, August 11, 1888.

[2] Brit. Med. Journ., January 28 and June 16, 1888, and Med.-Chir. Trans., vol. lxxi.

[3] N. Y. Med. Record, February 9, 1889.

has pointed out the occurrence of these connective tissues in cases of caries, and one of the specimens exhibited in connection with this paper is a tumor of this character.

The case of Gowers and Horsley was a man, aged forty-two years, and the operation was performed June 9, 1887. He had suffered for three years from localized pain beneath the left scapula. The pain varied considerably. He showed great mental irritability and hypochondria. Four months before operation first the left and then the right leg became weak, and gradually there was complete paraplegia. He had motor and sensory paralysis up to the level of the sixth or seventh dorsal nerves, with intense spasm in the legs, with foot clonus and rectus clonus. Urine was retained and he had some cystitis. He suffered with severe pain around the trunk, which was greater on the left side, and increased to agony by any movement. Caries and syphilitic disease of the spine were excluded. The spines and laminæ of the fifth, fourth, and third dorsal vertebræ were removed, and a fibro-myxoma, about the size of a filbert, was discovered and removed. Mr. Horsley reported, January 4, 1888, that the pain was for a time slightly relieved, but again and again returned with great severity, and the power of motion was only slowly and intermittently regained during the first three or four weeks. The local pain gradually diminished and motor power gradually returned. The surgical result was excellent. He reported, June 12, 1888, that the patient had lost the agonizing pain, and at the same time gradually recovered motor and sensory power, as well as the control of the bladder and rectum. He remains in perfect health.

One of the most recent contributions to the subject of spinal trephining that of Dr. Abbe,[1] is a case of extradural tumor of the spine, with complete paraplegia, in which the operation resulted in recovery.

The patient was a man, aged twenty-two years, and the operation was performed on May 25, 1888. The first symptom was pain in the back; swelling of the soft parts two inches wide by three long. There was disturbance of tactile sensibility in both legs, followed by gradually increasing paresis. The line of hyperæsthesia was between the anæsthesia and the normal skin. He had uncontrollable twitching of the legs, which were cyanotic when dependent. The spine became rigid and very painful; the hyperæsthesia, anæsthesia, and paralysis deepened. He suffered with girdle pains about the limiting line of the disease. He had incontinence of urine and feces. Active hectic was present. The spines and laminæ of the eighth, ninth, and tenth vertebræ were cut away, and a dense mass of tissue and desiccated pus was removed by scraping. Sensation began to return on

[1] N. Y. Med. Journ., November 24, 1888, and N. Y. Med. Record, February 9, 1889.

the eighth day; in the fourth week he began to move his left leg and toes on the right. The fever and pain disappeared, and in six weeks he moved both legs. He improved continuously, and in four and a half months he moved, pushing a chair. He was exhibited to the Surgical Society of New York eight months after the operation, when he walked well without support and was stout and heavy.

In the disease described by Charcot and Joffroy as hypertrophic internal pachymeningitis, operation for the removal of the lesion might in some cases prove successful. Usually after the disease has existed for some time the lesion becomes practically a fusiform tumor of considerable size. It might be objected, perhaps, that the disease is inflammatory in character; but, on the other hand, it is chronic, often of slow development, and sometimes almost stationary. No harm could result in a disease always one of great suffering and eventually fatal, and great relief and possibly a cure might sometimes be effected.

INTRASPINAL HEMORRHAGE.

As within the cranium, so within the spinal canal, hemorrhage may be of four varieties as to location,—namely, (1) supradural or extrameningeal; (2) subdural; (3) subarachnoid or subspinal; and (4) intraspinal,—that is, into the spinal cord itself. The hemorrhage may be posterior, anterior or lateral, or antero-lateral, postero-lateral, etc.; but commonly it will surround the cord; if intraspinal, it may be into the anterior or posterior horns, the central gray matter, or columns, but usually it will be diffused through the cord. With reference to the question of trephining it is only practical to consider supradural or subdural hemorrhage.

A net-work of large bloodvessels lies between the dura mater and spinal bone, and from this supradural spinal hemorrhage usually occurs. Such an extravasation may be minute, of moderate size, or very extensive. The symptoms of supradural hemorrhage will vary, of course, with its location and extent. The history is generally of a sudden attack, often after a fall or exhibition of violence of some kind.

Hemorrhage between the dura mater and the bone probably usually finds its way down the whole length of the spinal canal to the beginning of the sacral canal. The space surrounding the cauda equina in the lumbar canal is comparatively large, and the much-narrowed sacral canal is placed at a slightly different level, so that much of the effused blood would probably accumulate in the lumbar region. The fat and connective tissue between the theca and the bone are loosely distributed in the extradural space, and differ considerably in amount in different individuals, so that in some cases more than in others much of the blood would be entangled and coagulated in the meshes of the connective tissue and fat near the seat of extravasation. The subdural space, although much smaller than that outside of the dura, is a smooth serous cavity, and therefore much of a hemorrhage at any level would also probably descend into the region of the conus and cauda equina.

I do not know of any reported case in which trephining has been resorted to simply for the relief of the intraspinal hemorrhage. In a case of resection at the Bellevue Hospital, New York, by Dr. Stephen Smith,[1] it is reported that notwithstanding from eight to twelve ounces of extravasated blood, having a dark color, escaped from the spinal canal after the depressed bone had been extracted, the compression of the cord from extravasated blood and the paraplegia steadily crept upward, and finally caused death by asphyxia. The case is reported to show that an operation of trephining the spinal column or resecting the vertebræ would not relieve the spinal cord from compression when it was exerted by the extravasation of blood.

The diagnostic symptoms of meningeal hemorrhage as given by Gowers and others, and according to my own experience, are as follows: (1) sudden and violent pain in the back, less or more diffused; (2) pain along the course of the nerves passing through the membrane near the extravasation; (3) abnormal

[1] Lidell, in the Intern. Encycl. Surg., vol. iv. p. 379.

sensations,—tingling, etc., and hyperæsthesia, referred to the same parts; (4) spasm involving vertebral and other muscles supplied by affected nerves and also sometimes the muscles supplied by the cord below the seat of the hemorrhage; (5) sometimes general convulsive movements; (6) sometimes spasmodic retention of the urine; (7) consecutive paralytic symptoms, but not usually complete paralysis.

Some points of differential diagnosis between meningeal hemorrhage and extravasation into the substance of the cord should be borne in mind. Symptoms of irritation, such as pain, hyperæsthesia, paræsthesia, and spasm, in meningeal hemorrhage are usually immediate or very early, and may precede paralysis, which is commonly not complete. In hemorrhage into the substance of the cord paralysis may be very complete at first, or rapidly become so, and symptoms of irritation may be very largely wanting. Hemorrhage may, and not infrequently does, involve not only the membranes but also the substances of the cord, giving complex symptoms.

In some cases of intraspinal hemorrhage, whether primary or secondary, trephining might be performed in two places in order to make sure of a good result,—namely, at the supposed seat of the extravasation, and at the lowest portion of the lumbar spine, where, as already indicated, much of the extravasated blood would gravitate. Kronlein, it will be remembered, has recommended that in cases of cranial meningeal hemorrhage, two trephinings should be performed if one is not successful, and several cases of failure in trephining for intracranial hemorrhage have been reported, which would have been successful had the second operation been performed. In like manner, two trephinings of the spine, or a single trephining at the lowest possible place in the lumbar region, might prove successful where failure would otherwise result.

The somewhat frequent occurrence of spinal hemorrhage, with fracture of either the body, spines, or laminæ of the vertebræ, must not be overlooked. The diagnosis of the exact height of the lesion and the decision as to trephining may often have to be settled negatively or positively through a considera-

tion of the question of the probability of a double lesion in cases of accident,—namely, both fracture and hemorrhage. The fracture and the hemorrhage may be situated at different levels, or if the hemorrhage occurs primarily at the seat of the fracture it may find its way down the canal.

Many illustrations might be given gathered from medical and surgical literature of intraspinal hemorrhage in which operation would probably have proved successful, but I will confine myself to two.

Jackson[1] reported an interesting case of localized spinal apoplexy, which with our present lights might perhaps have been relieved and a life saved by careful trephining.

The patient was a bright girl, fourteen years old. While dressing her fingers felt weak. The next day she had a similar weak feeling in her hands. One day later she was unable to move her arms except at the wrists. Still later, the intercostal muscles did not act quite freely, and she seemed to lie heavier in the bed. Moist crepitant râles, with a little cough, developed. On the fifth day after the first symptoms, careful examination clearly demonstrated great loss of power in all the voluntary muscles of respiration and in those muscles of the arm, back, and chest supplied by the cervical nerves. The diaphragm became fixed, and there was slight lividity about the cheeks, with a fall in the natural temperature. Sir W. Jenner, who was called in consultation, diagnosticated a clot in the cervical portion of the spinal cord. Death soon took place without pain. The whole cervical portion of the spine, but particularly in front and to the left, was embedded in an oblong clot of dark venous blood outside the membranes. The clot ceased at the seventh cervical vertebra.

In this case, as the reporter remarked, the effusion probably took place very gradually, had room to extend itself, and coagulated very slowly and imperfectly. Until the phrenic nerves were interfered with, nearly every symptom might have been attributed to hysteria.

The following case was reported by Dr. Arlidge to the Staffordshire branch of the British Medical Association.[2]

The patient, forty-four years old, a drinking man, was admitted to the infirmary with complete motor paralysis of the legs, and some weakness in

[1] Lancet, July 3, 1869.

[2] Med. and Surg. Rep., March 23, 1889, vol. lx. No. 12, p. 370.

the arms, where, too, formication and numbness were experienced. Sensation was normal, and the legs were painless and their reflexes abolished. His only complaint was of dorsal pain. The bladder was distended, and prior to admission had been emptied by the catheter, but there was no dribbling of urine. Some rhonchi were noted in the upper part of the left lung; heart healthy; bowels confined for four days; the cerebrum undisturbed. The first temperature taken was 103.6°, but the next day it fluctuated between 105° and 106°, and rose to 107.4° the day preceding his death, on September 26th, and was accompanied by some delirium. A post-mortem examination revealed a copious hemorrhage within the meninges of the spinal cord, extending downward from the last cervical vertebra for about six inches. The blood was coagulated. No ruptured vessels were found in the cord itself, and no cerebral disease, although the dura mater was very adherent to the skull. There was no fracture of the spine, and no caries of any vertebra.

CARIES AND ANGULAR CURVATURE.

The question of operation for the relief of angular curvature of the spine is one of considerable importance and not to be dismissed hastily. Such an operation has been, until lately, commonly regarded as useless or worse. Its dangers have been overrated, although they are doubtless somewhat greater than most cranial operations. Macewen has reported an extraordinary series of six cases, in which elevation of the posterior laminæ of the vertebræ was performed—in four with recovery. In favor of operation are,—(1) the fact that by thus allowing the cord to expand backward a present serious paralysis may sometimes be relieved without imperilling the ordinary chances of recovery by anchylosis; (2) when an approximate cure has been effected by anchylosis, but paralyses or contractures remain, the latter may sometimes be relieved and improved; (3) secondary degeneration of the cord may sometimes be prevented or arrested; (4) when the paraplegia is produced by the connective tissue or other tumors which sometimes accompany angular curvature, rather than by the latter, such growths can be removed. Against operation are, —(1) that cases can sometimes be cured without such heroic procedure; (2) that caries often involves the body of so many vertebræ that operation is often useless or worse.

Trephining in such cases has been performed twice at the Philadelphia Hospital by Dr. White in cases of Dr. Dercum.

A case is reported by Mr. G. A. Wright,[1] of a boy, seven years old, who was operated upon January 26, 1888. Disease of the spine was observed one month before operation, with marked angular curvature in the mid-dorsal region, with some lateral thickening. Paresis of the lower limbs was present, and the patient grew gradually worse. Before the operation he had complete paralysis of both lower extremities, with incontinence of urine and feces. Anæsthesia to the level of the eleventh rib on the right side, and on the left decrease and finally loss of sensation, developed, the anæsthesia extending from the toes to the level of the pubes; from thence to the line of the eleventh rib a condition of analgesia existed. The muscles of the lower limb were in a state of tonic contraction, with marked flexion. The plantar reflexes were exaggerated; cremasteric very feeble, and deep reflexes could not be obtained. His feet were slightly swollen and cyanotic. Until late the symptoms were more marked on the right side than on the left. External and internal treatment were used without avail. Three laminæ were removed in the dorsal region. The theca of the spinal cord exposed was surrounded by a buff-colored, tough, leathery substance, which was cut away with the scissors. The cord did not appear to pulsate, but no point of constriction could be found. Some improvement took place in voluntary motion and sensation during a month following the operation. Later, the area of anæsthesia increased, and the condition of the patient became about the same as it was before the operation.

SPINAL ABSCESS.

Spinal abscess, like brain abscess, is usually a complication found in connection with disease of the bodies of the vertebræ, and localization rules will not commonly be of much value in making a diagnosis. Pus at any level within the spinal canal may, like blood, find its way to the lumbar region. In rare cases suppuration is confined to the space between the dura mater and bone, and when our diagnostic acumen becomes greater we will doubtless in some of these cases be able to trephine with advantage to our patient. Pachymeningitis, with or without suppuration, is generally due to traumatism or vertebral caries, but occasionally cases appear to arise as primary or idiopathic.

[1] Lancet, July 14, 1888.

In this so-called idiopathic suppuration in the spinal dura mater, trephining low down to give exit to the pus might in rare cases prove a useful procedure. Such a case is that reported by Dr. Robert Maguire.[1] Other cases have been reported by Mueller, Spencer,[2] and others.

In Spencer's case of idiopathic inflammation of the spinal dura mater, the patient's illness began with pain in the feet and knees and in the small of the back, frequently shooting down the back to the legs. He had fever and diarrhœa. There was great tenderness on pressure over both lumbar regions. The surface of the body was covered with copious acid sweats. Temperature elevated; pulse rapid, wiry, regular; tongue dry, white fur in the centre, red at the tip and edges. Breathing became rapid and somewhat painful, with a dry, hacking cough. The patient became delirious and his pain in the back very severe, but no paralysis until late. The neck was very stiff. There was a peculiar purplish mottling on the chest and abdomen. He became steadily weaker and died. The whole spinal canal was filled, from within two or three inches of the head down to the sacrum, with thick, creamy pus of a light yellow color and containing numerous shreds of lymph. The pus was found to lie outside the dura mater, between it and the periosteum. The pus had found its way out of the canal through the intervertebral foramina, and surrounded the spinal nerves for some distance. It had also passed out through the laminæ and infiltrated the connective tissue between the muscular structures on each side of the spine, forming collections of various sizes, the largest opposite the lumbar vertebræ. These were not abscesses, but infiltrations of pus into the interstices between the muscles. The tissues around these collections of pus were softened and the spinal nerves were laid bare. The most noteworthy of these collections was one opposite and to the right of the sixth and seventh dorsal vertebræ; the pus had here found its way under the costal pleura and projected into the thorax as a tumor, the size of a large hen's egg, lying close to the spine.

In this case trephining low down, as well as operations on the external abscesses, might have been performed.

NEURALGIA.

In rare cases of intractable neuralgia, trephining and section of the nerves close to the spinal cord may be tried as a last resort. This operation has been performed in one case by Dr.

[1] Lancet, July 7, 1888.

[2] Ibid., June 14, 1879.

Abbe,[1] of New York, on the recommendation of Dr. C. L Dana.

The patient, a man forty-four years old, was operated on December 31, 1888, and January 2, 1889. His symptoms began with throbbing pain on the posterior surface of the right forearm above the middle; later, paroxysmal pain, giving a peculiar twitching sensation in the thumb, index, and middle fingers; after some months there was complete disablement of the hand, which he kept in a stiff position with the fingers semiflexed. The forearm and hand were slightly emaciated, with atrophy of the muscles in the interosseous spaces. The posterior interosseous and ulnar nerves were stretched without any improvement. The arm was amputated about the humeral insertion of the deltoid, but without abatement of pain. The morphine habit had been contracted. He had twitching and tonic contractions of the muscles of the trunk; also the Brauch-Romberg symptoms; he swayed in standing and had a tendency to fall to the right. Knee-jerks were exaggerated, and there was ankle-clonus in the right leg. The right shoulder showed muscular atrophy, and stiffness in the neck muscles. A small tender neuroma of the muscular nerve was removed without relief. The right half of the laminæ of the third cervical to the first dorsal spine was removed. Both motor and sensory roots of the sixth and seventh cervical nerves were cut square across outside the dura. The posterior roots of the seventh and eighth cervical nerves were cut off close to the posterior columns. The patient made great improvement.

In the second part of this paper I will treat of some of the rules and principles for localizing spinal lesions for operative or other purposes.

Part II.

THE FACTS AND RULES OF SPINAL LOCALIZATION.

In a paper on "Cerebral Localization,"[2] I directed attention to the possibility of six classes of symptoms presenting themselves for the consideration of the neurological diagnostician —namely, those of (1) local irritation, (2) local destruction, (3) local pressure, (4) invasion by lesions growing from adjacent areas to those under determination, (5) local instability, (6) re-

[1] N. Y. Med. Record, February 9, 1889.

[2] Transactions of the Congress of American Physicians and Surgeons, First Triennial Session, Washington, 1888, page 238.

flex action at a distance. All or any one of these classes might require to be considered in localizing a spinal lesion, but the last three are not relatively of as much importance in spinal as in encephalic diagnosis; and, in order to restrict the present paper to reasonable limits, the discussion of this part of the subject will practically include only a study of symptoms of irritation, destruction, and pressure. Irritative, destructive, and compression lesions, whether intracranial or intraspinal, give rise mainly to three great classes of diagnostic phenomena —reflex, sensory, and motor. Vaso-motor, trophic, visceral, thermic, psychic, and some special symptoms, have at times considerable importance, but are generally secondary in value for ordinary practical purposes to the three classes mentioned.

Under reflex phenomena are usually included the cutaneous reflexes—scapular, epigastric, abdominal, cremasteric, gluteal, plantar, etc., and the so-called tendon reflexes—wrist-jerk, knee-jerk, ankle-jerk, ankle-clonus, perineal jerk, etc. Under the same head, also, come such phenomena as the involuntary reactions of the rectum, bladder, and sexual organs. Reflex symptoms may be, as might be expected from the functions of the spinal cord, of decided localizing value in spinal lesions, whereas for brain lesions they are usually only of general significance and value. Such reflexes as the scapular, epigastric, abdominal, cremasteric, gluteal, plantar, and rectal may, for instance, greatly help to fix the height of a lesion, as may also such phenomena as knee-jerk, muscle-jerk, and ankle-clonus, but the latter are sometimes in spinal as in cerebral cases only of general significance.

Some of the so-called reflexes are a guide to definite spinal segments. While, however, it is important and a great aid to prompt diagnosis to recognize quickly certain marked isolated symptoms, as disturbance of particular reflexes, on the other hand, it is essential that such phenomena should not be misinterpreted. Exaggeration of certain reflexes, as, for instance, the ankle-jerk, knee-jerk, the cremasteric jerk, etc., indicates not merely disease producing irritability of the centres for these phenomena, but, it may be, rather disease or compression

in the cerebro-spinal axis anywhere above these centres. Abolished knee-jerk may be indicative of extensive degenerative disease of the spinal cord, as in posterior sclerosis; or it may, in much rarer cases, indicate limited disease of the particular segment of the cord related to the patellar tendon. Westphal,[1] for instance, has reported the autopsy of a case of dementia paralytica, in which the patellar reflex had been absent on the left side only; and in which sections of the cord showed extensive sclerosis of the posterior root zones at the junction of the lumbar and dorsal regions on the left side, while on the right was only a slight beginning of the sclerotic process.

Under sensory phenomena it is necessary to consider clearly the particular variety of sensation lost or perverted, the exact cutaneous areas affected by increase, depression, or perversion of sensation, the nerves involved, the situation of their nuclei in the cord, and the relations of their origin to the intervertebral foramina. These sensory phenomena include the symptoms—pain, hyperæsthesia, anæsthesia, and paræsthesia.

The great motor symptoms are paresis or paralysis, spasm, contracture, and tremor. In studying motor phenomena, we must consider not only the muscles affected by loss or exaltation of power, the exact nerve-supply to such muscles, and the intraspinal course of these nerves, but also the situation of the nuclei of these nerves in certain segments of the spinal cord, even the particular cell-groups to which such nuclei belong. The value of a study in spinal localization depends largely upon the *exactness* with which we differentiate such phenomena and relate them to their lesions.

A good practical point is the importance of some single striking fact in spinal function in localization diagnosis for surgical or other purposes. That the phrenic or diaphragmatic spinal centre is situated in the fourth, or in the third and fourth cervical segments, should, for instance, be taken into full consideration in operations in this locality, great care being

[1] Ann. Univ. Med. Sci., vol. i. p. 95, 1888.

taken not to bruise or deeply injure the cord, or to explore with needles or by incision a region where such a vital centre has its abiding-place. Respiratory and cardiac phenomena occur with lesions at and above the phrenic centre; pupillary changes when the cord is affected above the level of the second dorsal nerve or segment; while extreme trophic or vaso-motor affections of the skin and its appendages are only present when the central gray matter, somewhat posterior to the central canal, is diseased.

My main object in this portion of the present paper is to give the methods of referring or relating sensory, motor, reflex, and other phenomena, which are distributed in limited regions of the body, to the particular levels of the spinal cord in which they have their source or termination. It is necessary, for both the physician and surgeon, to have quick and ready methods of determining the position of a lesion in the spinal cord by a study of the symptoms presented. The study is a broad one, and, in considering it, a paper might be almost indefinitely extended; it is necessary, therefore, to seize upon salient features and methods of presentation.

SOME POINTS IN COARSE SPINAL ANATOMY.

Certain well-known anatomical facts should always be borne in mind when considering for practical purposes the question of spinal localization. We should remember that the spinal cord proper is only from seventeen to eighteen inches long, while the spinal canal to the last lumbar vertebra is about twenty-three inches, and, if the sacral portion and coccyx are included, four inches more should be added. The cord, therefore, is much shorter than the canal, and usually reaches only to about the second lumbar vertebra. As already indicated in Part I., one of the most important and most available positions for successful spinal surgery, in cases of localized lesion, is the region of the cauda equina, most of which is entirely below the spinal cord. We have, in other words, a comparatively extensive intraspinal region, in which

the lesions are, strictly speaking, peripheral, and the diagnosis, both local and general, must be made with this fact fully recognized. That the spinal cord does not fill its canal transversely is also a significant familiar fact. The cord is, also, suspended very loosely in the canal. It follows, therefore, that many of the intraspinal lesions which are operable are membranous or bony in origin, and in symptomatology are, in part at least, peripheral as they involve nerve-trunks in their intraspinal course, however long or short this may be. Every one who wishes to become practically familiar with spinal anatomy for surgical purposes should study a series of transverse sections showing the relations of the spinal cord to its enclosing envelopes, membranous and bony. Even the dural sheath is much larger than is necessary for its contents. I have already referred to the fact growing out of such anatomical studies of the spine, that the spinal cord can be lifted with care so as to be the subject of operation on any of its surfaces, anterior or lateral, as well as posterior. A consideration of all the questions relating to the spinal membranes and spaces will eventually enable us to separate with fair accuracy the lesions which are supradural, subdural, and, strictly speaking, spinal.

For various reasons, operations for localized lesions, next to the cauda equinal region, probably offer more chances of success in the dorsal spine than elsewhere, owing to the less size of the cord and the less danger in operating. In the cervical and lumbar enlargements the greater bulk of the cord and the closeness of the nerve-roots are always to be considered. From one part of the cord, the smallest in transverse section, some of the largest nerves of the body originate. This is the narrowed lowest extremity of the cord, the conus medullaris, in which the lower sacral nerves have their roots. It is a region small in general bulk, because, owing to its position, it contains few conducting tracts, although relatively a large amount of gray matter. Lesions of this conus medullaris, embedded in its network of great nerves, can be accurately made out by symptoms, and its actual and

relative size to the membranes and spaces of the spine should, therefore, always be kept in view.

The intraspinal course of the spinal nerves should be absolutely familiar. Nearly all of the nerve-roots descend to the foramina, from which they emerge to be distributed to various parts of the body. The lowest nerves have, of course, the longest intramural course. Some facts about the relations of spinal segments to the vertebræ, foramina, and nerves are, doubtless, known to all, but are probably not much considered. The segmental nature of the spinal cord will be referred to presently at some length. The term spinal segment is used to indicate the limited vertical portion of the cord from which arises each pair of spinal nerves. In the cervical region are eight of these segments, but only seven vertebræ; hence the eight pairs of spinal nerves emerge from their foramina in such a manner that, although they all descend a little in their course, they emerge after the first so that the segments are located opposite spines and bodies of the vertebræ which do not correspond in name. The third cervical segment, for example, according to Gowers, is about opposite the first cervical spine, and its nerves emerge between the second and third cervical bodies. The eighth cervical segment is about opposite the sixth cervical spine, or the junction of the sixth and seventh, and its nerves emerge between the seventh cervical and first dorsal body. In the dorsal region the segments are the longest of the entire cord, and these segments are not situated numerically opposite spines and bodies of the same name in any single case. The first dorsal segment is nearly opposite the seventh cervical spine and body; the second dorsal nearly in a line with the first dorsal spine and body; the tenth, eleventh, and twelfth dorsal segments are much shorter than the segments from the first to the ninth, and these three segments are included chiefly between the eighth and tenth dorsal spines and bodies. The entire eleven spinal segments, from the first lumbar to the coccygeal inclusive, are crowded vertically between the eleventh dorsal and second lumbar spines and bodies, a space of only about two to three inches.

Commonly, we speak only of two sets of nerve-roots—the anterior or motor, and the posterior or sensory; the famous discovery of Sir Charles Bell, however, really divided the nerve-roots throughout the central nervous system into three sets, the third being a lateral or respiratory set, and containing nerves which excite motions which depend on or are related to the act of respiration. Gaskell[1] says that physiologists have failed to follow because the triple arrangement of these nerve-roots was not immediately evident, like the separation of the anterior and posterior roots. This lateral tract of nerve-roots is the same as the non-ganglionated part of the splanchnic root.

These lateral roots do not concern us much with our present lights in determining questions of localization. When, however, visceral and vascular localization have been more perfected, the question of the existence and involvement of these lateral groups will receive greater attention. Every segment of the spinal cord, according to Gaskell, gives origin to two roots, a somatic and a splanchnic—the latter being the roots of the nerves supplied to the viscera and bloodvessels; the former to the muscles, skin, and tissues other than those which are vascular and visceral. The somatic roots are, in other words, the motor and sensory, or the anterior and posterior roots, and are connected respectively with the cells of the anterior and posterior cornua. The lateral roots arise from two columns of nerve-cells—namely, the column of Clarke and a column in the lateral horn. The splanchnic root arising from the column of Clarke is ganglionated, while that arising from the lateral horn is non-ganglionated; and the nerves corresponding to the latter go to muscular tissues chiefly. Lesions of the splanchnic roots, or of the cell groups or columns of the cord with which they are connected, give rise chiefly to vasomotor, trophic, secretory, and visceral phenomena, distributed in accordance with the segment of the cord or the special pair of nerve-roots implicated. These lateral roots are found in greatest number in the region from the second thoracic or dorsal segment to the first lumbar.

[1] Journ. Physiol., vii. 1–80. London, 1886.

SEGMENTAL CHARACTER OF THE SPINAL CORD.

One of the first steps toward an understanding of spinal localization is the full recognition of the segmental character of the spinal cord. It is to be regarded not as a single organ, but as a series or succession of organs vertically linked together—a chain of segments one placed above another. In many of the lower animals, as in fishes and snakes, this arrangement is obvious, the spinal cord being made up of a series of alternating swellings and constrictions. "Each segment," says Bramwell,[1] "may be viewed as a distinct spinal unit, or, to speak somewhat figuratively, as a distinct spinal cord for a definite area of the body—viz., that portion of muscle (muscular area) to which its anterior roots proceed, and that portion of skin, tendon, muscle, mucous membrane, viscus, etc. (sensitive area), to which the fibres of its posterior nerve-roots are distributed. Now, the essence of the clinical examination of the spinal cord consists in the separate and systematic examination of each spinal segment by observing the motor, sensory, reflex, vaso-motor, and trophic conditions of the body area." According to Professor Hill,[1] of Cambridge, the recognition of the segmental succession in the arrangement of the nuclei of the nerves of the spinal cord is due to the late Professor Aeby. In the Hunterian Lectures, in 1885, Hill applied this principle of segmentation to the brain, or, at least, to the cranial nerves, attempting to fix the position in the cerebral axis of the nuclei of these nerves by a consideration of their segmental distribution in the head.

LOCALIZING PHENOMENA REFERABLE TO THE CONDUCTING TRACTS IN THE SPINAL CORD.

The spinal cord of man, besides being a great centre, or collection of centres, includes also a conducting medium, through which the great tracts (motor, sensory, etc.) pass, to connect

[1] Diseases of the Spinal Cord, p. 1. Edinburgh, 1884.
[2] Brain, January, 1888.

the brain with all the segments situated below, and these segments with each other. In man, the conducting tracts may be regarded as developmental additions to the original spinal segment. It is in its capacity as a centre—in the capacity of each segment as a special centre—that we are particularly concerned in studying spinal localization, but the symptoms produced by irritation, destruction, or compression of the conducting tracts will often greatly modify the opinion as to the exact height, and particularly the extent of a lesion. I cannot, of course, go into a lengthy consideration of the functions of the white substance of the cord. This can be found in the treatise of Gowers, or any good work on nervous diseases. For completeness, however, I will summarize the subdivisions of the cord and their functions, and make some practical applications to the subject of localization, in the main following Gowers in giving the functions of the different subdivisions. A transverse section at different heights would give a somewhat but not greatly different appearance. In a portion of the cervical region the greatest number of subdivisions are present. Starting here with the posterior median portion of the cord—the part which would be first exposed by the surgeon's knife and saw—the posterior column is divided into two parts—the *column of Goll*, or *postero-median column*, and the *column of Burdach*, also known as the *postero-external column*, or *posterior root zone*. The column of Goll in all probability conducts tactile impressions. The column of Burdach, containing, as it does, the posterior root-fibres, has for its function the conveyance of sensory impressions through these root-fibres, and probably also for short distances it carries these sensory impressions vertically. Just external to the periphery of the posterior horn begins a most important conducting tract—*the pyramidal tract*, or, as it is usually called, *the lateral or crossed pyramidal tract*—which occupies the posterior half of the lateral column outside of the posterior horn. Its function is unquestionably chiefly motor conduction, particularly from the brain to the parts below the arm. A column, shallow from without inward, but wide circumferentially, is the *direct cerebellar tract*, which lies

chiefly between the lateral pyramidal tract and the periphery of the cord. Flechsig believes that this column conducts impressions from the muscles of the trunk. When diseased it degenerates upward, and therefore its function must be to conduct upward. It is probably connected with Clarke's vesicular column, and therefore with visceral and vascular sensibility and control. Between the pyramidal tract and the concavity formed by the junction externally of the anterior and posterior horns is a comparatively narrow space called *the lateral limiting layer*, whose fibres may connect the gray matter at the different levels. What Gowers has termed the *antero-lateral ascending tract* occupies about the same position in the antero-lateral columns as the direct cerebellar tract does in the postero-lateral column. Its function is believed by Gowers to be the conduction of the sensations of pain and temperature. In disease it certainly degenerates upward, and therefore must in health conduct upward. Journeying still toward the front of the cord and the median line, we find that the antero-lateral white substance is very largely occupied by what is now called the *column of the anterior ground-fibres*, a name given by Flechsig. The anterior root-fibres traverse it, and its function is probably also to connect the anterior horns at different levels, and through the anterior commissure the anterior horns of the two sides of the cord at the same or at different levels. Like the posterior root zone, the ground-fibres of this column do not degenerate through any considerable vertical length of the cord. Lastly, the *anterior pyramidal tract, or column of Türck*, is a narrow tract lying close to the anterior median fissure, sometimes varying in form and size, and having for its function the conduction of motor impressions from the brain to the arm. If this is its function, its fibres must cross in the cord.

It is evident from this brief statement of the subdivisions of the white substance of the cord that, by closely bearing them in mind, the diagnostician will be greatly helped in locating the vertical position of a focal lesion, compressive or destructive, or both. Such a consideration may enable us to say, for instance, whether such a lesion is anterior, posterior, or lateral,

or how much it occupies any one or more of these positions; or, finally, a matter of great importance, as pointed out by Horsley, the mode of extension of a unilateral lesion, by a tumor which eventually becomes bilateral.

One of the oldest, and, at the same time, one of the best, illustrations of the importance of bearing in mind the functions of the various columns or tracts of the cord is afforded by the affections known as spinal hemiplegia and spinal hemi-paraplegia, which are due to focal lesions confined to a lateral half of the spinal cord. In the first place, such a lateral lesion is modified by its height in the cord. If comparatively high up in the cervical region and completely unilateral, the patient would exhibit motor paralysis of both the arm and the leg on the side of the body corresponding to the seat of the lesion, because of the implication of the column of Türck or direct cerebral tract, and the lateral or crossed pyramidal tract, which both decussate above the lesion. Sensation would be affected chiefly on the side of the body opposite to the seat of the lesion, because of the involvement of the antero-lateral ascending tract and the column of Goll, and because also the sensory tracts decussate in the cord itself. From involvement of the direct cerebellar column, vaso-motor and visceral symptoms would be present, chiefly on the same side as the lesion. The temperature is sometimes higher, for instance, on this side. The pupil may be affected through the cilio-spinal centres. This is simply the gross, sketchy picture of a lesion high up in the cord. The phenomena would vary considerably according as the lesion was chiefly compressive, irritative, or destructive.

A unilateral focal lesion of the dorsal cord would give hemi-paraplegia. In such a case the arm, the pupil, and the neck escape. Motor paralysis occurs in the leg of the same side, and anæsthesia in the trunk and leg of the opposite side. Sometimes a band of hyperæsthesia, or even widespread hyperæsthesia, is present on the side of the lesion, due doubtless usually to irritation of nerve-roots.

Instead of this somewhat complete symptom-picture, we may have a set of symptoms which are dependent upon involvement

of only one or two of the subdivisions of the white substance of the cord, although such exceedingly circumscribed lesions are comparatively rare. By the close study of history and symptoms we can sometimes trace the progress of a lesion from one tract to another.

Some peculiarities in sensory symptoms may occasionally give the clue to the possible extent of a lesion. The loss of the tactile sense, with the preservation of the senses of pain and temperature, for example, mentioned in one of the cases in the first part of this paper, was possibly of some value in connection with the question of the localization of the spinal paths for the transmission of the sensations of touch, pain, and temperature. Schiff's and Gowers's view, as stated, is that the lateral columns contain the tracts for sensibility to pain and probably also to temperature, and the posterior columns the paths for the conduction of tactile sensibility. In a case of focal lesion with compression, like the one reported, the existence of tactile anæsthesia, without loss of sensation of pain and temperature, would seem to be most easily explained on the supposition that the paths for these different sensations are not in exactly the same position in the cord. The columns for touch being the most posterior, the tactile anæsthesia may have been due to counter-pressure from jamming backward of the cord by the vertebral displacement.

Let me say a few words about the vertical extension of the spinal column. The column of Goll, or postero-median column, extends throughout the whole length of the cord. The postero-external column, or column of Burdach, posterior root zone of Charcot, continues throughout the entire cord, but degenerates only a short distance above a lesion. The direct cerebellar tract only reaches from the level of the first lumbar nerve upward, chiefly from the region of the vesicular column of Clarke; it does not, for example, degenerate from a lesion of the lumbar enlargement. The lateral limiting layer, between the lateral pyramidal tract and posterior horn, is throughout the cord. The fibres of the antero-lateral ascending tract of Gowers, often confounded with the cerebellar tract, pass up-

ward through the whole length of the cord. The anterior ground-fibres also extend throughout the whole length of the cord. The column of Türck is present only in the cervical and upper dorsal cord. The lateral pyramidal tract extends through the whole length of the cord.

The importance of knowing the vertical extension as well as the position of the spinal tracts for practical purposes is at once evident, and in diagnosis comes out with great distinctness in localizing focal lesions of very limited extent. Such a lesion might, for example, involve only or chiefly the columns of Türck, or anterior pyramidal tract, and would, therefore, give us motor-conducting symptoms, largely confined to the arms. If the lesion was lateral or peripheral, it might only or chiefly involve the ascending antero-lateral tracts, and give us for symptoms interference with sensibility to pain and temperature, on one or both sides. The same reasoning can be readily extended to other isolated or neighboring tracts.

In considering a lesion below the upper dorsal region the practical diagnostician would not take into account the column of Türck, which has no existence below this level. The direct cerebellar column would also be out of consideration in large measure in lesions of the lumbo-sacral cord and cauda equina. On the other hand, the columns of Goll and of Burdach, the crossed pyramidal tract, the antero-lateral ascending tract, and the region of the anterior ground-fibres would be considered at any and all heights.

SEGMENTAL LOCALIZATION.

Theoretically, it is possible to localize a lesion in any segment of the cord. It is practically possible to do this in many segments of the cord, and for some the process with our present light is by no means difficult. For any vertical subdivision of the cord containing two or three segments we should be able to localize lesions with sufficient accuracy for grave operations.

In the detailed study of segmental localization, the order in which the subject will be considered is (1) sensory localization;

(2) motor localization; (3) reflex localization; (4) the localization of vaso-motor and trophic phenomena. Regarding the cord as made up of a series of reflex systems, or reflex nerve-roots or arcs, sensory impressions are conveyed inward from the peripheral end-organs to sensory roots and centres; thence are transferred to motor centres, and thence outward again reflexly to roots, nerves, and muscles themselves; so that the study of a single segment of the cord will include sensory, motor, and reflex phenomena. When the brain takes part the sensory impression enters the cord by the posterior nerve-roots, and passes up the opposite side of the cord to the cerebrum, and eventually calls forth a motor impulse, which is sent down the cord to the anterior motor cells, and thence to the muscles.

SEGMENTAL SENSORY LOCALIZATION.

The sensory mechanism of the cerebro-spinal system consists, therefore, of peripheral end-organs which receive impressions, incoming nerve-fibres which carry them by way of the posterior roots into the spinal cord, commissural fibres which immediately or almost immediately transfer them to the opposite half of the cord, and ascending conducting tracts by which they are conveyed up the cord to the oblongata, pons, crus, internal capsule, corona radiata, and brain cortex. Various interrupting ganglia are interposed along this sensory highway; but these we shall not here consider. Dividing one-half of the spinal cord will cause anæsthesia in the opposite half of the body below the place of hemisection, and at the same time a line or belt of hyperæsthesia or anæsthesia may be produced on the same side as the lesion in the distribution of some particular nerve-root or roots injured. This has already been referred to in considering spinal hemiplegia and hemi-paraplegia. This anæsthesia is in the main caused by severance of the ascending conducting paths for sensation, which are in the side of the cord opposite to the parts of the body supplied. It is necessary, however, to consider more particularly the sensory effects in certain territories and their special subdivisions, because of

the implication of one or more segments, or limited vertical extensions, of the spinal cord.

Segmental sensory localization has not been much elucidated by the contributions of physiologists, who have done so much for motor localization. The contributions to this subject are, indeed, few and of quite recent date. The most valuable have been furnished by Ross[1] and Thorburn;[2] and by Herringham,[3] Paterson,[4] and Goodsir,[5] who are quoted by Ross. The limits of this paper will allow me only to refer briefly to this subject, chiefly giving the facts determined by Ross and Thorburn. Heiberg's[6] *Atlas* has proved of value in my studies of the various sensory areas of the skin. The papers from which the most useful data have been obtained have been clinico-pathological and embryological—studies of cases of injury to the spinal cord, intraspinal nerve-trunks, and extraspinal nerve-trunks and plexuses; and investigations chiefly developmental and morphological of the limb plexuses of the lower animals and of man.

A spinal nerve, as Ross shows, is derived from the cord by a posterior (superior) gangliated and an anterior (inferior) non-gangliated root. Where the two roots come together the nerve divides into a superior primary division supplying the skin of the back over the neural canal; and an inferior primary division, which subdivides into a dorsal trunk, supplying the lateral, and a ventral, supplying the ventral or anterior surfaces. A branch is also given off to the viscera.

The entire surface of the body may be divided into three

[1] Brain, April, 1884; ibid., January, 1885; ibid., January, 1888.

[2] Brain, January, 1887; ibid., January, 1888; ibid., October, 1888; Brit. Med. Journ., December 22, 1888; and Med. Chron., April, 1889.

[3] Proc. Royal Society, vol. xli., 1887.

[4] Journ. Anat. and Phys., April, 1887; ibid., July, 1887; and Quart. Journ. Micr. Sci., vol. xxviii., 1887.

[5] Edin. New Phil. Journ., N. S., vol. v., January, 1887, and Anat. Mem., vol. ii. Quoted from Paterson by Ross.

[6] Atlas of the Cutaneous Nerve-supply of the Human Body. By Jacob Heiberg, M.D. Illustrated by Alfred Fosterud. Translated and edited by W. W. Wagstaffe, B.A., F.R.C.S.

great nerve territories—one posterior, one lateral, and one anterior or ventral.

The sensory nerve-supply to the posterior territory is comparatively easy to understand. It is from the superior primary divisions of the spinal nerves. It occupies a region which may be roughly described as consisting of the back of the head and the back—the region over the neural canal. Heiberg puts its lower limit as roughly indicated by the crest of the ilium inside of the sacrum. The back of the head is supplied from the upper cervical nerves, because in the process of evolution the skin of the back of the neck has been dragged upward in order to cover the greatly enlarged brain and skull of the man. This posterior nerve territory in the head reaches forward over the top of the head and forehead to the root of the nose, its anterior portion being supplied from the superior primary or first division of the trigeminal or fifth nerve. Ross compares the distribution of the sensory nerves in the dorsal or posterior region of the cord with that which is seen in the lowest vertebrata—the amphioxus, for example. In the amphioxus section of the posterior roots of a pair of nerves will cause anæsthesia of exactly corresponding sections of the body. In man section of the dorsal posterior roots (superior primary division) likewise produces anæsthesia of the skin of corresponding segments of the body, the only differences being that in man some anastomoses of different dorsal nerves occur, and the nerve areas of distribution tend to be placed successively lower with reference to the levels at which the nerves originate.

The anterior nerve territory of the trunk in a manner corresponds to the posterior territory—that is, it occupies a position on the front of the trunk and face comparable in extent to that of the posterior territory on the back and head. The lateral territory is the largest and in many respects the most important sensory surface. Heiberg places the line of demarcation between the lateral and anterior territories as generally dividing the breast into two equal parts, the distribution of the anterior branches, however, extending on to the front of the

thighs. The extremities are largely supplied by the lateral branches. On the neck the branches are all anterior and posterior; in the thorax the anterior and lateral cutaneous branches are from the same nerves.

The limb plexuses are formed from the union of the branches of the inferior primary divisions of the nerves. The sensory nerves from the two branches of this inferior primary division are very irregularly distributed in the limbs—that is, looking at the matter from a gross topographical point of view. This is due to the fact that, in the process of development of the limbs in the evolution of higher from lower animal forms, the sensory nerves, both dorsal and ventral, have been irregularly arrested in the extremities. In the formation of the plexuses the dorsal branches nearly always unite with the dorsal, and the ventral with the ventral branches only; although Paterson, according to Ross, admits exceptions in the case of the small sciatic nerve, which is formed by a union of dorsal and ventral fibres, and in that of the external and short saphenous nerves which are undoubtedly so constituted.

In the limbs, then, the sensory disturbances which are due to involvement either of the cervical or lumbo-sacral segments of the spinal cord, or of the nerve-roots or nerves springing from these segments, are distributed more or less longitudinally in accordance with the particular segment or nerve affected. The hyperæsthesia, for example, which in a dorsal lesion extends around the trunk in the form of a belt or girdle, in a cervical or lumbo-sacral lesion has a longitudinal distribution more or less regular in the limbs. In like manner, the anæsthesia and paræsthesia present in the limbs have distributions in accordance with the shape of the cutaneous sensory areas. A close knowledge of sensory distribution is therefore necessary that the diagnostician shall quickly be able to decide on the spinal or other localization of a lesion by the zone of disturbance in the limb.

Sensory segmental localization follows certain laws. If the upper extremity, according to Ross, be placed in the embryological position—that is, with the thumb directed outward and

upward, the palm forward toward the front of the body—the preaxial border from the tip of the shoulder down to the metacarpo-phalangeal articulations of the index-finger and thumb is supplied by the fifth cervical root, and the postaxial border, from the axilla to the finger-tips inclusive, is supplied by the humeral branch of the second, the first dorsal, and the eighth cervical nerves. Observations made in cases of disease of the cauda equina have also convinced Dr. Ross that, if the lower extremities be also placed in the embryological or tailor position, the preaxial border is supplied by the cutaneous nerves of the four upper lumbar nerves, and the postaxial by the coccygeal and sacral sensory nerves. He has concluded, also, that the most distal parts of the preaxial border were supplied from the lower of the four lumbar nerves, and of the postaxial border by the higher sacral nerves.

The chief law of sensory distribution, as worked out by Herringham and adopted by Ross, is as follows:

"A. Of two spots on the skin, that which is nearer the preaxial border tends to be supplied by the higher nerve.

"B. Of two spots in the preaxial area the lower tends to be supplied by the lower nerve, and of two spots in the postaxial area the lower tends to be supplied by the higher nerve."

While many cases of disease limited to the anterior horns or to particular cell-groups of these horns have been recorded, I do not know of any reports of lesions circumscribed in the posterior horns of particular segments of the cord. Most of our practical knowledge of sensory segmental localization is therefore to be derived from cases which have been reported by Ross, Thorburn, and others, in which both the cord and nerve-roots have been injured extensively. To the papers of these observers I will refer those interested for details, giving here only some of the summarized conclusions.

Thorburn,[1] for example, in one of his papers on spinal localization as indicated by spinal injuries, drawing his observations from nineteen cases of injury to the cervical region of the

[1] Brain, October, 1888.

spinal cord, speaks as follows with reference to the distribution of anæsthesia in the upper limbs:

"Dr. Ross has fully demonstrated the distribution of the sensory nerves in the upper limb. Regarding the limb in its embryological position, we find that it is projected as a bud from the trunk, the hand being supine and the radius upward, so that the palmar surface is anterior. In this position the bud carries out with it branches of the anterior primary divisions of the spinal segmental nerves from the fifth cervical to the first dorsal inclusive; and as these nerves maintain in the adult their embryological relations, we have the several roots supplying the limbs in numerical order from the radial to the ulnar side. Hence, then, the higher the paralysis extends in the cord through the brachial region, the farther will the anæsthesia extend from the ulnar toward the radial side. This arrangement is fully demonstrated by our cases. It is necessary to refer again to the distribution, which will be obvious to any one who takes the trouble to read the reports, and of which I have already spoken in my previous paper. It will be found that the fifth root supplies the region overlying the deltoid muscle, and the outer aspect of the arm and forearm as far as the styloid process of the radius or base of the thumb, and that the eighth cervical and first dorsal supply the little finger and inner side of the hand, forearm, and arm, the remaining roots providing for the central parts of the limb on both anterior and posterior aspects. It will, however, be found that these central roots supply relatively a much less extensive area than those above and below them."

In his paper on injuries to the lumbo-sacral region,[1] Thorburn likewise summarizes the sensory distribution of the various lumbo-sacral nerve-roots. This summary is as follows:

"First lumbar nerve: Ilio-hypogastric and ilio-inguinal.

"Second lumbar nerve: Outer (?) and upper part of thigh.

"Third lumbar nerve: Anterior aspect of thigh below second lumbar.

"Fourth lumbar nerve: Anterior part of leg.

"Fifth lumbar nerve: Back of thigh, except in distribution of first, second, and third sacral.

"First sacral nerve: A narrow strip on back of thigh; back of leg and ankle; sole; part of dorsum of foot.

"Second and third sacral nerves: Perineum, external genitals, 'saddle-shaped' area of back of thigh."

MOTOR LOCALIZATION.

Methods anatomical, physiological, microscopical, clinico-pathological, and clinical have all contributed their share to

[1] Medical Chronicle, April, 1889.

our knowledge of motor localization; and to some of the results obtained in each of these ways I will briefly refer.

ANATOMICAL RESEARCHES.

Herringham carefully and patiently dissected the nerves of the brachial plexus on many subjects, fœtal and older, in order to trace these nerves to their final destinations in the muscles and the skin. He gives a tabular statement showing the usual nerve-supply of muscles of the upper limbs, as follows:

"Third, fourth, and fifth cervical: Levator anguli scapulæ.

"Fifth cervical: Rhomboids.

"Fifth, or fifth and sixth cervical: Supraspinatus, infraspinatus, teres minor.

"Fifth and sixth cervical: Subscapularis, deltoid, biceps, brachialis anticus.

"Sixth cervical: Teres major, pronator teres, flexor carpi radialis, supinator longus and brevis, superficial thenar muscles.

"Fifth, sixth, and seventh cervical: Serratus magnus.

"Sixth and seventh cervical: Extensor carpi radialis.

"Seventh cervical: Coraco-brachialis, latissimus dorsi, extensors at back of forearm, outer head of triceps.

"Seventh and eighth cervical: Inner head of triceps.

"Seventh, eighth, and ninth cervical: Flexor sublimis, flexor profundus, carpi ulnaris, longus pollicis, and pronator quadratus.

"Eighth cervical: Long head of triceps, hypothenar muscles, interossei, deep thenar muscles."

PHYSIOLOGICAL RESEARCHES.

Few experimental investigations with reference to the exact functions of the nerve-roots and horns of the spinal cord have been made; but one of the most important of these was that of Ferrier and Yeo in 1880.[1] This investigation was as to the effects of irritation of the motor roots of the brachial and crural plexuses of monkeys. The faradic current was employed. It was found that each motor root represented a distinct functional combination. The muscles set in action were so related as to bring about a definite action of an adapted

[1] Proc. Roy. Soc., London, No. 212.

nature. The muscles affected by electricity exciting any one root were related in function, and this relation was not simply one of contact or nearness.

The actions produced by the different roots were summarized by Ferrier as follows:

"First dorsal: action of the intrinsic muscles of the hand, muscles of the ball of the thumb, interossei, etc.

"Eighth cervical: closure of the fist, with pronation and ulnar flexion of the wrist, retraction of the arm, with extension of the forearm.

"Seventh cervical: the *scalptor ani* action—viz., adduction with rotation inward and retraction of upper arm, extension of forearm and flexion of wrist and fingers, so as to bring the tips against the flank.

"Sixth cervical: the movement of 'attention'—viz., adduction and retraction of upper arm, extension of forearm, pronation and flexion of wrist, the palm of the hand being brought toward pubes.

"Fifth cervical: movement of the hand toward the mouth—viz., raising the upper arm inward, flexion of the forearm with supination, and extension of the wrist and fingers.

"Fourth cervical: a similar movement of forearm and hand—viz., the upper arm is raised upward and backward."

The actions of the *lower extremity* were respectively:

"Second sacral: action of the intrinsic muscles of the foot—viz., adduction and flexion of the hallux, with flexion of the proximal phalanges and extension of the distal.

"First sacral: flexion of the leg, plantar flexion of the foot, flexion of all the toes at the proximal phalanges, and also of the distal phalanx of the hallux.

"Fifth lumbar: outward rotation of the thigh, flexion and inward rotation of the leg, plantar flexion of the foot, and flexion of the distal phalanges.

"Fourth lumbar: extension of the thigh, extension of the leg, and pointing of the great toe.

"Third lumbar: flexion of the thigh and extension of the leg."

Paul Bert and Marcacci,[1] in 1881, studied the distribution of the motor roots of the lumbar plexus, their researches being made on dogs and cats. The nerves were very carefully cut close to their origin, and stimulated electrically with the utmost care.

[1] The results obtained by Bert and Marcacci were summarized in the Lancet of October 1, 1881.

"It was found that the first root of the lumbar plexus determines the contraction of the sartorius, the rectus, and the psoas muscles, which are closely connected in the dog and cat, and all of which flex the hip upon the trunk. The second root of the plexus excites contraction in the anterior portion of the vastus externus, a part of the tensor of the fascia lata, and the vastus internus—*i. e.*, in the muscles which extend the leg on the thigh. The function of the third root is similar to that of the second, with some differences in detail. It excites part of the vastus internus, the anterior part only of the biceps, which is an extensor, while the posterior portion is a flexor. The fourth root causes movements in the posterior part of the biceps, the semitendinosus and the semi-membranosus (flexors of the leg and thigh), and the second and third adductors of the thigh, and the extensors of the thigh. It thus innervates three kinds of movements, which are in no respect opposed or contradictory. The fifth root presides over the movements of the tail. From these results the experimenters conclude that there is evidently a systematic arrangement of the innervation of the limb at the origin of the nerves of the spinal cord; it is a functional systematization—*i. e.*, the motor filaments arising at a given level are distributed to muscular masses, which act together, and concur to produce an associated movement. In the second place (as Duchenne long ago demonstrated and as is now generally recognized). the anatomical unity of a muscle has no physiological correspondence. A single muscular mass may be in one part a flexor, in another an extensor. But the nerve distribution corresponds to the function, in so far as the different roots innervating the several functions are concerned."

Various researches, which are referred to by Ferrier and Yeo, have been made on the lower animals with a view to determine the situations of the roots of the plexuses of the limbs. Peyer and Krouse found that most of the muscles of the limbs were supplied by more than one root of the brachial plexus. It was determined that the muscles nearer the shoulder were supplied by the higher roots and those of the hand by the lower root of the plexus; also that the sensory roots have a distribution corresponding to that of the anterior roots.

During the operation by Dr. Abbe[1] on Dr. Dana's case of intractable brachial neuralgia, several interesting experiments were made with the faradic battery; so far as I know, the first of the kind on man. With a sponge electrode on the back a

[1] Medical Record, February 9, 1889.

metal-point electrode was applied at various points. When applied to the sixth nerve, just external to the dura, it caused contraction of the supraspinati and infraspinati, rhomboid, latissimus dorsi, pectoralis major, teres, and deltoid. Application to the seventh nerve caused contraction of the pectoral, latissimus dorsi, and abductors of the arm; to the eighth nerve similar contraction and pain.

MICROSCOPICAL INVESTIGATIONS: CELL-GROUPS WHICH REPRESENT PHYSIOLOGICAL UNITS IN THE HORNS OF THE SPINAL CORD.

Microscopical investigations show that in cross-sections of the spinal cord a distinct transverse localization of cell-groups which are related to certain definite muscles or muscular groups concerned in particular movements may be readily determined. The cord is not only an organ composed of a series of segments placed one above the other, but these segments differ in size both vertically and horizontally. The gray matter particularly shows marked differences in shape and size in various transverse sections. These differences depend largely upon the number and grouping of the cells.

Starr, drawing from various investigations, details at length the manner in which in the anterior horns of the cord the cells are arranged in distinct groups which can be determined and enumerated from within outward. He gives a series of diagrams which show the manner not only in which these cells are arranged in clusters at certain levels of the cord, but also how these continue or fail to continue in a vertical direction from one level to another.

These cell-clusters differ in number and in relative position at various heights in the cord. In the second cervical segment, for example, are three, two continuing downward into the third segment of the cord, the third even into the fourth segment. In the fourth cervical segment the number of groups is five. Again, in the fourth lumbar segment, and below this as far as the sacral, three groups are present. These groupings in the cord are enumerated and described by Starr, and also

by Gowers and Ross. The only point to remember in connection with practical localization is that each of these groups is probably a physiological unit. They are spinal centres of function comparable to the cerebral centres of motor function. Spitzka, in 1880, first advanced this opinion :

"The nearer the muscle is to the ventral aspect of an animal," he said, "the nearer will its nucleus be to the median line of the cord ; and the nearer the muscle is to the dorsal aspect of the animal, the nearer will its nucleus be to the lateral cornu of the cord. Flexor nuclei are therefore in internal, and extensor nuclei in external and posterior cell-groups. Thus the cell-group in the apex of the anterior horn as well as in the lateral cornu is fairly developed in the dorsal region, while the portion of the gray substance situated internally is deficient in cells, in evident relation to the deficiency of prevertebral muscles. To the extremities of the body, lateral extension of the cornua correspond, and in these the same relative position of flexor and extensor nuclei is probable. As is shown in the enlargements, there is much more differentiation of the cell-groups in such extensions in the multidigitate animals than in the solipeds, as is shown by comparing the cord of man with that of the horse. Whether groups of muscles be flexor or extensor, it will be found that the nearer they are to the animal axis, the nearer will their nuclei be to the central canal. This is especially true of the flexor nuclei. The increasing development of a cell-group in the upper cervical region near the central canal in animals with powerful head and neck flexors seems to support this statement. Extensors remote from the axis, such as the trapezius, have their nuclei quite remote from the central canal." (Quoted by Starr.)

The diagnosis of a spinal lesion in the horns or columns of the cord may sometimes be at least of negative importance in deciding the question of operation. This was illustrated in the case of Lloyd and Deaver of a cyst or extravasation of the cervical region of the cord. The lesion was found to be a hemorrhagic extravasation in the anterior horns of the cord, practically out of reach of the knife. In this case, however, an external swelling, which made operation important, was present. If the external indications had not been present, the close diagnosis of the exact location of the lesion in a transverse section of the cord would have been of great importance. We have now data on hand for such localization at different levels. We can even make a fairly exact diagnosis of the

position of a lesion at various positions from within outward in the anterior horns of the cord.

It may even be found that our knowledge of the exact position from without inward of these groups of cells which represent physiological units—which are, in other words, related to certain definite movements—may occasionally prove of practical value. Decision as to operating, for example, may depend not only upon our knowledge of the exact level and vertical limitations of a lesion, but also, to some extent, upon the amount of compression and of destruction by invasion of the spinal cord. As has just been stated, the nuclei of extensor muscles are situated in external and posterior cell-groups, while those of flexor nuclei are more internal; therefore, the predominance of extensor or of flexor paralysis may help sometimes to a conclusion as to the amount of transverse damage of the cord.

CLINICO-PATHOLOGICAL AND CLINICAL INVESTIGATIONS.

As already stated, Thorburn has published certain clinical and pathological observations upon injuries to the cervical region of the spinal cord, and has endeavored to draw therefrom an accurate picture of the functions of each of the nerve-roots entering into the formation of the brachial plexus. His usual method consisted in noting accurately the distribution of the paralysis and anæsthesia which ensued immediately upon a lesion of the cord, of which the exact site had been determined by most-mortem examination; also of pursuing the changes which resulted from the consequent ascending myelitis. He has also applied the same methods to the lumbo-sacral or crural plexus.

He has tabulated provisionally, as follows, what he is led, from the evidences of his cases, to believe to be the arrangement, from above downward, of the muscle nuclei in the cervical cord:

"Fourth cervical nerve: Supraspinatus and infraspinatus, teres minor (?).

"Fifth cervical: Biceps, brachialis anticus, deltoid, supinator longus, brevis (?).

"Sixth cervical: Subscapularis, pronators, teres major, latissimus dorsi, pectoralis major, triceps, serratus magnus.

"Seventh cervical: Extensors of the wrist.

"Eighth cervical: Flexors of the wrist.

"First dorsal nerve: Interossei, other intrinsic muscles of the hand."

He has likewise summarized for the lumbo-sacral region of the spinal cord the arrangement of both the motor and sensory fibres of the nerve-roots. His summary, he states, is to be distinctly understood as only that for which his own cases gave evidence,—that is, they are positive evidence for the facts reported, but do not negative the existence of other facts the existence of which was not proved by the cases. Thus, for example, although no doubt the second lumbar root affords muscular branches, these are ignored because his cases yield no evidence of the same. The sensory arrangement has already been given. The following is the motor distribution as determined by him:

"First lumbar nerve: None.

"Second lumbar: None.

"Third lumbar; Sartorius, adductors of thigh, flexors of thigh.

"Fourth lumbar: Extensors of knee, abductors of thigh.

"Fifth lumbar: Hamstring muscles.

"First and second sacral: Calf muscles, glutei, peronei, extensors of ankles, intrinsic muscles of foot.

"Third sacral: Perineal muscles (erector penis, transversalis perinei, accelerator urinæ, etc.).

"Fourth sacral: Bladder and rectum."

Ferrier,[1] in a paper on atrophic spinal paralysis, has applied the facts determined by his and other experiments, and made a provisional enumeration as a guide to further more minute clinical research, of the muscles which are likely to be affected in poliomyelitis limited to each segment, the muscles being placed in the order in which they would probably suffer.

"First dorsal type: The intrinsic muscles of the hands, viz., muscles of the thenar and hypothenar eminences and interossei.

"Eighth cervical type: Long flexors, ulnar flexors of wrist, intrinsic mus-

[1] Brain, vol. iv. p. 226, 1881–1882.

cles of hand, extensors of wrist and phalanges, long head of triceps (pectoralis major?).

"Seventh cervical type: teres major, latissimus dorsi, subscapularis, pectoralis major, flexors of wrist and fingers (median), triceps.

"Sixth cervical type: Latissimus dorsi, pectoralis major, serratus magnus, pronators (flexor of wrist?), triceps.

"Fifth cervical type: Deltoid (clavicular portion), biceps, brachialis anticus, serratus magnus, supinator longus, extensors of wrist and fingers.

"Fourth cervical type: Deltoid, rhomboid, supra- and infraspinatus (teres major), biceps, brachialis anticus, supinator longus, extensors of wrist and fingers, diaphragm."

In the lower extremity:

"Second sacral type: Intrinsic muscles of the foot, strictly parallel to the first dorsal type.

"First sacral type: Muscles of calf (plantar flexors), hamstrings, long flexor of big toe, intrinsic muscles of foot.

"Fifth lumbar type: Flexors and extensors of toes, tibial muscles, sural muscles, peroneal muscles, outward rotators of the thigh, hamstrings.

"Fourth lumbar type: Extensors of thigh, extensor cruris, peroneus longus, adductors.

"Third lumbar type: Ilio-psoas, sartorius, adductors, extensor cruris."

On the principles of localization certain types of spinal paralysis have also been determined and named by others, as by Remak and Erb. Remak speaks of an upper-arm type of atrophic spinal paralysis, in which are jointly affected the supinator longus with the brachialis anticus and the biceps and the deltoid. The same author describes the forearm type, in which the extensors of the wrist and fingers and the muscles of the hand are paralyzed. He has also described three types of paralysis of the lower extremities.

"The first, in which the extensors and adductors of the leg are affected together, the sartorius muscle, however, escaping, although it is supplied by the anterior crural nerve, which also supplies the extensors; a second type, in which the ilio-psoas, sartorius, and flexors of the leg are affected together; and a third type, in which the muscles upon the leg and foot are involved with the exception of the tibialis anticus. He notices that the last-named muscle is rarely paralyzed with the other muscles, and that when it is affected, the other muscles moving the foot frequently escape." (Starr.)

SEGMENTAL LOCALIZATION OF REFLEX CENTRES.

Certain cells in the gray matter of the spinal cord preside over reflex action. These reflex actions, and the areas in which they are capable of being excited, are described in the more recent text-books. These centres are cutaneous or superficial, and muscular (or musculo-tendinous) or deep. The centres for these various reflexes are enumerated by Gowers as follows:

"Superficial reflex action: Plantar, second sacral; gluteal, fourth lumbar; cremaster, second lumbar; abdominal, sixth to eleventh dorsal (epigastric, sixth dorsal); scapular, fifth cervical to first dorsal.

"Muscle reflex action: Calf muscles (foot clonus), fifth lumbar and first sacral; knee-jerk, third and fourth lumbar; flexor digitorum, triceps, seventh cervical; biceps, supinator longus, sixth cervical."

An important set of reflex centres for localization purposes are those which preside over various actions more or less complex and involuntary. These include such as the following: Cilio-spinal, cremasteric, sexual, parturitional, vesical, and anal. Starr enumerates the position of these centres as follows: Cremasteric, first to third lumbar; vesical, third lumbar; rectal or anal, fourth lumbar. By other observers the cilio-spinal centre is placed in the lower cervical cord and dorsal, as far as the second or third dorsal vertebra. The sexual, or erection centre, has been placed without close localization in the lumbar cord, probably about the second or third lumbar segment. Korner has located what he terms the parturition centre at the level of the first or second lumbar vertebra. Those centres for complex reflexes which are really valuable for purposes of practical localization are the cilio-spinal, cremasteric, vesical, and anal centres; their position will be indicated in the table at the conclusion of this article. The latest investigations of Thorburn and others indicate that the centres for the bladder and the anus or rectum are in the extreme lower part of the cord; in fact, in the terminal portion of the cord, the region of the third, fourth, and fifth sacral and the coccygeal nerve roots.

VASO-MOTOR AND TROPHIC LOCALIZATION.

Vaso-motor and trophic centres are distributed throughout the entire vertical extent of the spinal cord. Those connected with the muscles are identical with or situated close by the corresponding motor nuclei in the anterior horns. Vaso-motor and trophic centres for the skin, nails, and joints also have a local habitation in the spinal cord. Pathological facts, as those, for instance, which have been obtained in studying syringomyelia, point to the central gray matter of the cord as the probable seat of these centres. By the central gray matter I mean here that which is situated around or near the central canal. In the posterior portion of this central gray matter it is probable that the vaso-motor and trophic centres for the skin, nails, and joints are situated, while more anteriorly are probably placed the trophic centres for the bones. A special collection of large bipolar cells is found in the postero-internal gray matter, and extends in the form of a continuous column from about the seventh cervical to the second or third lumbar segment. This collection of cells, first described by Lockhart Clarke, is usually designated as Clarke's vesicular column, and has been referred to several times in this paper. It is now almost established by various pathological reports, and the careful anatomical and microscopical investigations of Gaskell, that this column represents a series of vaso-motor and visceral centres. Communicating nerves have been traced to and from this vesicular column to the ganglia and fibres of the so-called sympathetic nervous system. When, therefore, vaso-motor and trophic phenomena are present these regions of the central gray matter must be considered.

In 1884,[1] and again in 1888,[2] Starr summarized in tabular form the functions of the various segments of the spinal cord. His last table represents fairly well the status of spinal localization. In concluding this article I will give this table of

[1] American Journ. Neurol. and Psych.

[2] The American Journal of the Medical Sciences, May, 1888.

Localization of the Functions of the Segments of the Spinal Cord.

Segment.	Muscles.	Reflex.	Sensation.
Second and third cervical.	Sterno-mastoid. Trapezius. Scaleni and neck. Diaphragm.	*Hypochondrium* (?) (third to fourth cervical). Sudden inspiration produced by sudden pressure beneath the lower border of ribs.	Back of neck and of head to vertex. (Occipitalis major, occipitalis minor, auricularis magnus, superficialis colli, and supraclavicular.)
Fourth cervical.	Diaphragm. Deltoid. Biceps. Coraco-brachialis. Supinator longus. Rhomboid. Supra- and infraspinatus.	*Pupillary* (fourth cervical to second dorsal). Dilatation of the pupil produced by irritation of neck.	Neck. Shoulder, anterior surface. Outer arm. (Supraclavicular, circumflex, musculo-cutaneous or external cutaneous.)
Fifth cervical.	Deltoid. Biceps. Coraco-brachialis. Brachialis anticus. Supinator longus. Supinator brevis. Deep muscle of shoulder blade. Rhomboid. Teres minor. Pectoralis (clavicular part). Serratus magnus.	*Scapular* (fifth cervical to first dorsal). Irritation of skin over the scapula produces contraction of scapular muscles. *Supinator longus* (fourth to fifth cervical). Tapping the tendon of the supinator longus produces flexion of forearm.	Back of shoulder and arm. Outer side of arm and forearm to wrist. (Supraclavicular, circumflex, musculo-cutaneous or external cutaneous, internal cutaneous, radial.)
Sixth cervical.	Biceps. Brachialis anticus. Subscapular. Pectoralis (clavicular part). Serratus magnus. Triceps. Extensors of wrist and fingers. Pronators.	*Triceps* (sixth to seventh cervical). Tapping elbow tendon produces extension of forearm. *Posterior wrist* (sixth to eighth cervical). Tapping tendons causes extension of hand.	Outer side and front of forearm. Back of hand, radial distribution. (Chiefly musculo-cutaneous or external cutaneous, internal cutaneous).
Seventh cervical.	Triceps (long head). Extensors of wrist and fingers. Pronators of wrist. Flexors of wrist. Subscapular. Pectoralis (costal part). Serratus magnus. Latissimus dorsi. Teres major.	*Anterior wrist* (seventh to eighth cervical). Tapping anterior tendons causes flexion of wrist. *Palmar* (seventh cervical to first dorsal). Stroking palm causes closure of fingers.	Radial distribution in the hand. Median distribution in the palm, thumb, index and one-half middle finger. (Musculo-cutaneous or external cutaneous, internal cutaneous, radial, median.)
Eighth cervical.	Triceps (long head). Flexors of wrist and fingers. Intrinsic hand muscles	.....	Ulnar area of hand, back, and palm, inner border of forearm. (Internal cutaneous, ulnar.)

Segment.	Muscles.	Reflex.	Sensation
First dorsal.	Extensors of thumb. Intrinsic hand muscles Thenar and hypothenar muscles.	.....	Chiefly inner side of forearm and arm to near the axilla. (Chiefly internal cutaneous, and nerve of Wrisberg or lesser internal cutaneous.)
Second dorsal.	.....	.....	Inner side of arm near and in axilla. (Intercosto-humeral.)
Second to twelfth dorsal.	Muscles of back and abdomen. Erectores spinæ.	*Epigastric* (fourth to seventh dorsal). Tickling mammary region causes retraction of the epigastrium. *Abdominal* (seventh to eleventh dorsal). Stroking side of abdomen causes retraction of belly.	Skin of chest and abdomen, in bands running around and downward, corresponding to spinal nerves. Upper gluteal region. (Intercostals and dorsal posterior nerves.)
First lumbar.	Ilio-psoas. Rectus. Sartorius.	*Cremasteric* (first to third lumbar). Stroking inner thigh causes retraction of scrotum.	Skin over groin and front of scrotum. (Ilio-hypogastric, ilio-inguinal.)
Second lumbar.	Ilio-psoas. Sartorius. Quadriceps femoris.	.....	Outer side of thigh. (Genito-crural, external cutaneous.)
Third lumbar.	Quadriceps femoris. Anterior part of biceps. Inner rotators of thigh. Abductors of thigh.	*Patellar* (third to fourth lumbar). Striking patellar tendon causes extension of leg.	Front of thigh. (Middle cutaneous, internal cutaneous, long saphenous, obturator.)
Fourth lumbar.	Abductors of thigh. Adductors of thigh. Flexors of knee. Tibialis anticus. Peroneus longus.	*Gluteal* (fourth to fifth lumbar). Stroking buttock causes dimpling in fold of buttock.	Inner side of thigh, leg, and foot. (Internal cutaneous, long saphenous, obturator.)
Fifth lumbar.	Outward rotators. Flexors of knee. Flexors of ankle. Peronei. Extensors of toes.	*Achilles tendon* (fifth lumbar to first sacral). Over-extension causes rapid flexion of ankle, called ankle clonus.	Back and outer side of leg; sole; dorsum of foot. (External popliteal, external saphenous, musculo-cutaneous, plantar).
First and second sacral.	Flexors of ankle. Extensors of ankle. Long flexor of toes. Intrinsic foot muscles.	*Plantar* (fifth lumbar to second sacral). Tickling sole of foot causes flexion of toes and retraction of leg.	Back and outer side of leg; sole; dorsum of foot. (Same as fifth lumbar.)
Third, fourth, and fifth sacral.	Gluteus maximus. Perineal. Muscles of bladder, rectum, and external genitals.	Vesical centres. Anal centres.	Back of thigh, anus, perineum, external genitals. (Small sciatic, pudic, inferior hemorrhoidal, inferior pudendal.)
Fifth sacral and coccygeal.	Coccygeus muscles.	.....	Skin about the anus and coccyx. (Coccygeal.)

Starr, with modifications based upon the results obtained by Herringham, Ross, Thorburn, and others. A study of this table, and of the various summarized statements which have already been given, will enable the diagnostician with approximate accuracy to localize the height of a lesion affecting the segments of the cord, or the spinal nerve-roots. In the column for sensation the cutaneous nerves are given in parentheses; this list is not complete, but it is hoped that it will be of assistance in sensory localization.

THREE CASES OF CARIES OF THE SPINE, WITH NOTES ON THE SUBJECT OF TREPHINING.

By JAMES HENDRIE LLOYD, M.D.

[Read March 6, 1889.]

FIRST CASE. CARIES OF THE BODIES OF THE THIRD, FOURTH, AND FIFTH CERVICAL VERTEBRÆ, WITH LATE GENERAL TUBERCULOSIS. DEATH, AND AUTOPSY.

History: The patient was a white boy, aged thirteen at his death. He had sustained an injury when three years old, by falling and striking the back of his head, followed in a few days by what was said to be spinal meningitis. Four months after the fall he began to have trouble in walking, but recovered the use of his legs completely. The hump in his cervical region was first noticed two years after the fall.

His condition on admission to the Home for Crippled Children, one year before his death, was as follows: He was quite paralyzed from his hips down. He had partial control of his bladder, but could not long resist the desire to micturate. The patellar reflexes were much increased, as is common in these cases, and he had ankle clonus. Sensation was not abolished. In other words, he had the symptoms of a partial transverse lesion of the cord, involving especially the motor tracts. The right arm was paretic, somewhat wasted and contractured. The left arm was normal. The condition of the right arm was evidently due to a destructive lesion of the anterior horn in the cervical enlargement presiding over the nutrition of the arm-muscles. The pupils were unequal, the right being constantly dilated and immobile, which was probably due to irritation of the cervical sympathetic. A bed-sore had formed over the sacrum.

Fourteen months after admission the patient developed tubercular meningitis to which he rapidly succumbed. Preceding this he had some spells resembling petit mal, and again localized spasmodic action of the right face muscles. His meningitis was indicated by headache, hebetude, a slow and irregular pulse, and a temperature range which was irregularly febrile. The unequal pupils persisted, but as they had existed so long prior to all symptoms of meningeal tubercle, I considered that they were possibly due to the

lesion in the neck. There was cough, with moist râles. Aphasia was not noticed.

The autopsy revealed the following conditions: There was a dense deposit of tuberculous matter about the left middle cerebral artery, and a sprinkling of tubercles over the left island of Reil and the posterior part of the third frontal convolution, accounting, no doubt, for the spasmodic movements in the right face. Tubercles were also found on the capsule of the spleen, scattered through both lungs, and in one kidney, in which several cavities containing pus and cheesy matter were observed. The bodies of the third, fourth, and fifth cervical vertebræ were soft and crumbling, and could be scooped out with the handle of the knife. At this point there was a sharply angular curvature, over which the cord was bent at a right angle. Within the knee of this angle, as it were, the cord was softened, and evidently much damaged by the compression and folding up of its structure; over the knee, or angle, the fibres were evidently in a much better state of preservation; although somewhat stretched, yet firm. The anterior horns and antero-lateral tracts had thus evidently suffered most, while the posterior columns were practically intact, thus explaining why in these cases motor functions are more interfered with than sensory.

I have thought it worth while to report this autopsy because it throws some light upon the nature of the damage to the cord in a class of cases for which surgical interference has been recently proposed, and in some cases practised. Independent of the tuberculous history of this case, which many of these cases illustrate (and which would, of course, by itself preclude the possibility of permanent relief), the peculiar nature of the structural changes in the cord would discourage hope of relief by trephining in cases similar to this. The cord was not damaged by pressure, but by folding or bending upon itself, and this condition would not likely be much, if any, relieved by cutting away the laminæ and spinous processes.

SECOND CASE. LONG-STANDING CARIES OF UPPER DORSAL VERTEBRÆ. ABSCESS OF THE LUNG. NOTES OF AUTOPSY.

E. L., aged twenty-one years, had been a patient in the Home for Crippled Children under the writer's care for six years. He had an angular curvature of the spine, which was firmly solidified at the time of his admission, and there was never any cause, up to his final illness, to suppose that any active disease process was occurring in it. About one week before his death he was seized with severe headache and fever, and rapidly grew very ill. The pulse

was very quick, and the temperature once rose to 105°. About twenty-four hours before his death he began expectorating a highly offensive, purulent sputum. The right lung gave negative results on auscultation and percussion. The left lung was slightly impaired in its resonance in the neighborhood of the hump.

The autopsy revealed an abscess cavity in the left lung in the apex, which was pushed back into the fossa caused by the curvature. This cavity was in direct contact or communication with the carious spine. The right lung was normal. It had but two lobes. The left lung in the region of the abscess was inflamed and congested, and contained foci of tubercular matter, also some very limited areas of pneumonic consolidation. The heart, spleen, mesenteric glands, and peritoneum were normal. At the seat of the curvature the cord was bent rather in the shape of a bow than at an angle. Pus escaped from the carious bone. A thick layer of exuded fibrine existed between the dura and the bone, behind the cord, running up into the cervical region, and down almost the entire length of the spine. The abscess cavity was found at the region of the curvature, a probe passing through into the thorax.

This case illustrates the fact that in patients in whom for years the curvature has been supposed to be firmly consolidated, and in a sense cured, a slow, chronic disease-process may be keeping up. This patient had no paralytic symptoms, which I ascribe to the fact that the cord was bent in a bow-shape rather than at an angle. In any proposed surgical interference, such as trephining the cord for paraplegic symptoms, it must be borne in mind that even years of exemption from all active inflammatory symptoms do not preclude the possibility of a bad local state of the bones.

THIRD CASE. DISEASE OF OR INJURY TO THE AXIS AND THIRD CERVICAL VERTEBRA.

M. R., aged thirteen, Italian. A patient in the nervous wards of the Philadelphia Hospital. *Family history:* Father living and in good health. Mother died in Italy, cause unknown. One brother living, age sixteen, healthy. Four brothers and one sister died in infancy: cause unknown.

Previous history: Patient had the ordinary diseases of childhood, from which she fully recovered. Three months before the first symptoms of paralysis, while carrying a stone on her head, she fell. She complained of pain in the back of the neck, which, however, soon passed away.

History of present disease: Eight months ago she fell out of bed, and became unconscious for a short time. When she recovered consciousness she

had diminished power in her legs. She was, however, able to crawl about on her hands and knees. The loss of power grew gradually worse, and extended to her arms, which she has been unable to move for the past two months. These symptoms were greatly increased after a cold bath which was given to her toward the end of January.

Present condition: Complete loss of power in the legs, and almost complete loss in the arms; she has still slight use of the flexors and extensors of the arm and forearm. Patient is able to move her head from side to side, and to move it forward and backward, but these motions are impaired, and the power to support the head is diminished. There is no pain or hyperæsthesia. Slight tendency to rigidity of muscles of back. No difficulty in inspiration. There are paroxysms of difficulty in voiding urine. Obstinate constipation. Exaggerated reflexes in lower extremities. Tendency to reflex spasm in both lower and upper extremities. There is a protuberance of the axis, more marked toward the left side of column. There is but slight tenderness on pressure over this lump. The deltoid and infra- and supra-spinatus of both sides are atrophied, and exhibit degenerative reactions. There is slight decrease in the faradic irritability in the muscles of the forearm. Sensation is unimpaired.

NOTE (two months later).—It may be a question whether we have not in this case a partial dislocation of the axis. I think there is a slight prominence at the back of the pharynx. There is no evidence of active inflammatory process, abscess, etc. The patient has improved remarkably with rest in bed, and recently with a suspension-apparatus ("jury-mast") for the head. The power in her arms is almost restored, and she can take a few steps.

TWO CASES OF SPINAL MENINGITIS AFTER CARIES OF THE VERTEBRA.

By J. H. MUSSER, M.D.

[Read March 6, 1889.]

In the discussion this evening of the various phases of spinal caries, my object in presenting notes of the two cases recently under my observation is simply to emphasize the etiological relations of such disease to inflammatory affections of the meninges, and not to enter into any detailed account of the clinical or pathological phenomena. That this relationship is well known goes without saying; and yet there are cases which, on the one hand, do not present the rational symptoms or physical signs of caries, while on the other hand the symptoms of meningeal inflammation may be most obscure. It is true, as the first case developed under observation, the picture of spinal meningitis became quite clear, while its presence was considered highly probable in the second, yet, in both, the antecedent or conjoint occurrence of spinal caries was not recognized. It is to ascertain and lay stress upon the reasons for overlooking this etiological antecedent that I ask your attention for a few moments.

Case I.—C. P., female, aged forty-eight, white, widow, domestic, was admitted August 19, 1889, into the Philadelphia Hospital. Her family history was good, and her personal medical history included only the occurrence of scarlet fever, measles, varioloid, and typhoid fever, at their usual time of occurrence in life. She has had five children, three of which died in infancy. Her personal habits were good. She had to work very hard. Three weeks before admission, severe pain began between the scapulæ, and travelled, she states, from thence, lingering several days in each locality, to the lumbar region, the hips, the abdomen, and then the chest.

When examined by me, with Dr. Dock and my resident physician, Dr. Keller, she was apparently suffering agonizing pain in the lumbar region. This was attended with great restlessness and such emotional phenomena as to appear to warrant the belief that it was muscular, and was associated with hysteria. Anodynes in extreme doses failed to give relief, but blistering appeared to relieve it in this place, only to be succeeded by similar pains between the scapulæ. Again, a blister appeared to chase it to the neck behind, where, however, it persisted, in spite of internal medication and external remedies.

No other phenomena of meningeal inflammation, either central or peripheral, were observed, although carefully looked for. There was no fever. The appetite was lost, and the bowels constipated. The urine was normal.

Aug. 30. Slight evening delirium was observed, continuing during the 31st. On the evening of this date paraplegia was discovered, with retention of urine. Loss of motion and sensation, with retention of urine, continued through September 1st, but on the 2d there was slight motion in the left leg, and some return of sensation and reflex responses. No further change was noted until the morning of the 5th, when rapid œdema of the lungs ensued, causing death.

At the autopsy, old but quiet necrosis of the lower cervical and dorsal vertebræ was found, causing a slight but appreciable deformity; with old and recent external spinal meningitis, and recent internal meningitis, most intense and with greater exudation of pus and lymph in the cervical and lumbar regions. In the latter region the right half of the cord was softened, and, as the specimen shows, torn in its removal. Sections and staining, by the ordinary and the Weigert process, showed the cord and the nerve trunks to be free from inflammation, presenting an appearance the counterpart of a cut in Gower's recent work.

Of course, the absence of many of the usual symptoms of meningitis was due to the fact that the nerve trunks were not affected.

As previously noted, the occurrence of meningitis and myelitis came into recognition, but the vertebral disease was overlooked.

This was due partly to the fact that the previous medical history, as is common to most hospital patients, was not known, but chiefly because its presence was not sought for. This sin of omission may be atoned for largely by the paucity of symptoms of spinal meningitis, the occurrence of which certainly would not have been overlooked had they been present. It is true the spine was examined, but not with the patient stripped and in the erect posture. On this account also, the deformity

was not recognized, and hence the obvious corollary is, not to neglect such method of examination.

CASE II.—Acute atlo-axial disease; acute external and internal spinal meningitis.

L. M., aged twenty-two, male, single, intemperate, carter, exposed to all kinds of weather. No venereal disease. Typhoid two years ago, and scarlet fever in childhood. Family history negative.

Admitted to Philadelphia Hospital November 7, 1888. One week before admission quit work on account of acute pain in the gluteal and sacral regions, attended with general muscular soreness. So very sudden was the onset that it was attributed to strain. Local applications relieved the pain, but it seemed to pass upward along the spine to the neck, where it remained stationary during the four days prior to entering the hospital. Loss of appetite from the first, with occasional vomiting for two days.

On examination, patient looked strong and was well nourished. Face anxious and flushed. Eyes not injected, but pupils contracted and sluggish. Head held rigidly bent forward, and when sitting up, falls forward more. Severe pain on movement of head, referred to back of neck and along the reddened, swollen part on the right side, above the clavicle. This swelling, extending three inches above the clavicle, includes the portion between the trapezius and the sterno-cleido-mastoid muscles; the anterior fibres of the former and the whole of the latter being tense and rigid. No other motor, and no abnormal sensory phenomena. Patient quiet, and answers questions well, but slowly. Heart normal; action weak and rapid. Lungs slightly congested at the right base. Urine contained a small amount of albumin, but no casts. Temperature 101.3°, pulse 132, respiration 42.

8th. Restless and sleepless all night. Mind clear until 12 M. to-day. Since then slight delirium, with picking and grasping at the bed-clothing. No headache. Pupils dilated. Bowels and urine moved naturally. No vomiting. Temperature, A.M. 100.3°, P.M. 101.3°, pulse 120, respiration 38. Neck the same.

9th. High delirium all night. No other change, except much weaker. Temperature 101.3°, pulse 144, respiration 40. Death at 11.30 A.M. of heart clot.

Autopsy twenty hours after death: Right atlo-axial joint the seat of inflammation, with extension to the deep abscess in the neck, and to the meninges of the spinal cord. Large soft clots, with pus and lymph, were found between the dura and the bone, the greatest amount being opposite the cauda equina. Here the inflammation extended through the dura, and the interior of it, with the pia, were the seat of inflammation and bathed with lymph and blood. On the exterior surface of the dura, covering the foramen magnum, considerable lymph and pus were found. Spinal cord normal. Brain structure

normal. Dura much injected ; pia mater opaque. Cerebral vessels normal. No effusion.

The previously noted abscess was below and behind the diseased articulation, and was large enough to contain a small lemon. The cellular tissue of the neck was invaded from this point. The viscera did not present any changes to account for the inflammation. Chronic parenchymatous nephritis; old pericarditis; atheroma of the aorta and the coronary arteries, fibroid change in the cardiac muscle, with enlargement of the organ, were the most marked lesions. The bones and their periosteum, and the joints not mentioned above, were not examined.

Remarks.—That this patient had spinal meningitis was our opinion, although disease of any articulation was not suspected during life. Not enough stress was placed upon the external swelling and redness above the clavicle, a consideration of which might have led to the supposition of cervical bone disease. Indeed, the cellular inflammation was thought to be part of an obscure septic process which included inflammation of the meninges. In the light of the autopsies we would submit for consideration the feasibility of opening the canal and draining in similar cases. Certainly there were no contraindications found that might have prevented successful treatment by surgical means.

SPINAL CARIES: OPERATIVE TREATMENT—RESECTION OF LAMINA, ETC.

By DE FOREST WILLARD, M.D., Ph.D.,
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[Read March 6, 1889.]

The *operative treatment of spinal caries* may be considered under three heads:

1. Evacuation of pus.
2. Erasion or removal of carious bone.
3. Vertebrectomy or excision of the lamina for the relief of pressure paralysis.

1. Evacuation of Pus.—Until very recently the expectant plan of treatment of spinal abscess was generally adopted by surgeons, since they had learned from the failures of their predecessors that septic poisoning and death were frequent results of interference.

At the present time, with thorough antisepsis, such results are rare, and we can with confidence open any abscess, acute or chronic, with the full expectation that benefit will be obtained by the evacuation of an injurious material, the retention of which in the body may lead to most serious consequences from bacillary or tuberculous infection.

That such evils do not more frequently occur from pus retention is due to the fact that nature attempts to escape the baneful influence by intrenching herself behind a firm wall of inflammatory tissue.

a. Aspiration. My own practice is to use the aspirator in cases of doubt, when the abscess is very small, and when it is

still very deep beneath the skin. After drawing off the pus, and noting carefully its consistency, etc., I then inject from 15 to 30 grains of iodoform dissolved in glycerine and distilled water, 1 to 3. This is allowed to remain, and assists in producing the object which I have specially in view in this preliminary tapping, namely, the strengthening and thickening of the sac wall. I do not practise hyperdistention of the sac wall with sublimate solution, as entrance of the more powerful agent into the circulation might occur.

b. Evacuation. Ten days later, or after a longer delay, provided the abscess does not become distended, I freely open the sac under strict antiseptic precautions, and draw off the pus thoroughly, but only as freely as it will run without pressure. A drainage-tube is then inserted and the sac is thoroughly irrigated with hot sublimate solution, 1 : 2000.

An abundance of sublimate dressings are then firmly placed about the wound, and are changed only when they become offensive. In post-pharyngeal abscess from cervical caries the irrigation is omitted.

2. Erasion or Removal of Carious Bone.—Treves, Boeckel, Israel, Reclus, Ashhurst, and others have practised the plan of cutting down upon the diseased vertebra and removing the diseased tissues with spoon, gouge, etc.

Treves[1] made his incision along the edge of the erector spinæ muscle, about two and a half inches from the lumbar spinous processes. The muscle was then drawn inward until the transverse processes were found, when the quadratus lumborum was divided and the psoas incised. The abdominal branches of the lumbar arteries may require ligation. Should a sinus exist, it may be followed down to the bone.

In the dorsal region it is obvious that the body of the vertebræ, where the disease is usually situated, cannot be reached without removing a section of one or more ribs.

To remove successfully all the disease would require the thorough exploration of the entire body of the bone, a proced-

[1] Med.-Chir. Trans , vol. lxvii. 113, 1884–5.

ure which one surgeon, who had even had experience in this particular method, abandoned during the operation.

The results of the operation are not favorable, as the cases have been only temporarily benefited, and several have promptly succumbed.

For drainage purposes it offers inducements, however, and in the lumbar or cervical regions it would be a simple matter to cut down in the course of a sinus and deposit a large drainage-tube in direct contact with the diseased bone. Irrigation and the speedy release of all broken-down tissues would then hasten the cure.

The objections to erasion are:

a. That a sequestrum is rarely present, and if so, would be situated in the body of the vertebra.

b: That it would be impossible to reach the body, save in the lumbar region, without the sacrifice either of the articular process or of a portion of several ribs.

c. In caries it would be almost impossible to judge of the exact amount of devitalized bone, and as in other osseous structures, the process of removal with gouge would certainly destroy the underlying layer of bone, and thus become a fresh source of osteitis.

In so far as good drainage is secured I most heartily approve of the practice, but as regards the complete removal of carious tissue, I have great doubts.

3. Laminectomy (so-called trephining of spine) for the relief of pressure upon the cord, is now awakening much interest from the profession.

Pressure may be occasioned—1st. By the sharp angle in front, resulting from the rapidly disintegrating bodies of the vertebræ; 2d. By inflammatory outpouring of ossific matter due to caries of the posterior part of the vertebral body without breaking down of the shell; 3. By connective tissue infiltration around the theca; 4. By a super-abundance of caseous material about the cord.[1]

[1] Hun: Albany Med. Annals, July, 1888, ix. 193. Ridlon: New York Path. Soc. Transac., 1887, 83.

Paraplegia from vertebral osteitis is ordinarily double in type, but when the encroachment upon the cord is lateral, the paralysis may affect only one leg. Motion or sensation may be either partially or totally affected, while anæsthesia, analgesia, and exaggerated knee, ankle, and other reflexes are present.

To Macewen undoubtedly belongs the credit of resuscitating the operation of systematically searching for and removing any existing lesion, although from the time of Ægineta, Bell, and Cooper it has been discussed. Only a few years since it was denounced as unjustifiable.[1] Macewen justly claims that he has demonstrated that "the spinal membranes and the cord itself can be exposed, and that neoplasms and encroachments upon the lumen of the canal may be removed therefrom without unduly hazarding life. The old objections that such operations were full of danger from hemorrhage and were unprofitable and unsuccessful, have certainly been thoroughly disproven, and we are now able to offer a measure of success to a class of hitherto hopeless cases."[2]

Of course, the operation will not be attempted until extension, rest, and fixation have failed to bring about relief. Fortunately, the measures recommended are usually successful, and we may confidently hope for marked improvement under their use.

The operation consists in cutting down upon the spinous processes in the region of the deformity, the incision being slightly to one side of the centre so that the resulting cicatrix will not be unduly pressed upon during recumbency.

All the soft tissues are then stripped bare with a periosteal knife, until the entire lamina is exposed. One-half or the whole of the arch may be removed as necessary, but even an opening upon one side gives ready access to the canal.

A half trephine, a saw with cutting edge upon its convex surface, a chisel, or a pair of angular cutting bone forceps or rongeur forceps may be used for making the section. When

[1] Ashhurst's Encycl. Surgery, vol. iv.
[2] British Med. Journ., Aug. 11, 1888.

cut through the lamina can be lifted off, and then the theca lies exposed. Sometimes the cord can be seen pulsating in its bony canal; again, it lies shrunken.

If the pressure has been due to an inflammatory growth, the connective tissue neoplasm may be dissected away with scissors from the theca, or the latter may require removal with the growth. Should the pressure have been due to bony growths, the ossific material will probably lie in front of the cord and should be searched for as far as safety permits, the cord being lifted with a blunt hook. All discovered portions of dead bone should be removed, and thorough drainage secured laterally, if possible. Even when the anterior bony projection can be neither discovered nor removed, the pressure upon the cord will be very greatly relieved, by the freedom allowed to it for expansion posteriorly. Much of the benefit derived from the operation is doubtless due solely by this relief of pressure. Erasion of diseased bone should be practised, if possible, and the most thorough antiseptic precautions during operation and in subsequent dressings observed. The superficial tissues should be sutured separately from the deeper ones. Hemorrhage may be controlled by forceps pressure, by ligation, or by packing with iodoform gauze. Immediate improvement is not to be expected.

In one of Macewen's cases motion was first noticed on the eighth day, in another it returned much more slowly. In Wright's case no change occurred until the twelfth day, when faint pin-pricks were noticed. In three weeks slight voluntary movement was present. A third case was also successful.

The operation, therefore, is proven to be perfectly feasible, and as safe both as regards the risks of hemorrhage and the exposure of the cord, as any serious operation in surgery. It has been shown that even the theca of the cord may be removed without undue risk, and improvement has taken place even in desperate cases. Both sensation and motion have returned, with control of sphincters and with spastic contractions relieved. It relieves pressure even when the cause is not removed.

Conclusions.—Viewing the operation of removal of the laminæ for the pressure paralysis of spinal caries only, we have six published cases; three were most strikingly and brilliantly successful, one was not improved, one died in a week, another died in a few months from general tuberculosis.

It is fair to presume that life was shortened in the speedily fatal case; in the second case it may or may not have hastened the result. One case improved for a few days, but in a month was precisely as before.

Of the three successful cases, in one the paralysis had existed for two years, and had resisted both extension and fixation. Both sensation and motion, with control of bladder and rectum, were lost, and the leg muscles were spastically contracted. Reflexes were exaggerated. The recovery was so perfect that he was able to walk in six months, and to play football five years later, although the laminæ of the fifth, sixth, and seventh dorsal were removed, and a fibrous neoplasm outside the theca was cut away.

In the second case, the loss of sensation and motion seemed hopeless, yet in four days she could retain feces and urine, and in eight months walked freely, although at the time of the operation the condition of the cord seemed hopeless, from the pressure of a dense connective tissue tumor between the bone and theca.

In the third case, the angle was sharp at the first dorsal. There was complete motor and sensory paralysis, with spastic contractions and absent reflexes. These reflexes returned to nearly normal condition subsequent to the operation, although entirely absent before, and the feces and urine could be controlled after the fourth day. In nine months the patient walked well.[1]

In Wright's[2] case (Thomson), in a mid-dorsal curve in a

[1] British Med. Journ., August 11, 1888, ii. 308, 323. Glasgow Med. Journ., 1884, xxii. 65. Glasgow Med. Journ., 1886, xxv. 210. Med. Contemp. Napoli., 1884, i. 520.

[2] Lancet, July 14, 1888, 264. Internat. Journ. Surgery and Antiseptics, October, 1888, 225.

boy of seven, the paralysis was complete, reflexes were increased, there was analgesia and the muscles were tonically contracted. The case grew worse under treatment by recumbency and iodides. The cord was found enclosed in a tough, leathery mass. At the end of a week there was slight return of motion and of sensation, but soon relapse followed, and at the end of three months the condition was unimproved.

In an unpublished case of Dercum and White's in Philadelphia, death speedily followed.

The lamina has also recently been removed with success by Macewen, for traumatic paraplegia;[1] and Jones[2] (Thorburn) reports a similar case.

The arch has also been cut away by Horsley[3] and Gowers for tumor, by Lloyd[4] and Deaver, by Abbe, and by Weir.

Abbe has also made the section for dislocation and for persistent brachial neuralgia.

A large number of operations for traumatism have been collected by Ashhurst.[5]

The arguments against the operation are:

1. It endangers life, and a certain percentage of cases will die from shock that would otherwise live for years and might even recover.

2. It is uncertain in its relief, since when the compression is anterior it may be impossible to remove the cause.

3. It weakens the only support of the head and shoulders, in the portion of the column upon which alone dependence is to be placed, since the anterior support—*i. e.*, the bodies of the vertebræ—has been already disintegrated.

This weakening process must throw additional strain upon both muscles and diseased bone, and the operation, if done before decided consolidation had occurred, would leave the

[1] Brit. Med. Journ., Aug. 11, 1888, 308. Glasgow Med. Journ., 1886, xxv. 210.
[2] British Med. Journ., Sept. 22, 1888, ii. 881.
[3] Brit. Med. Journ., 1888, i. 191, 1273.
[4] Amer. Journ. Med. Sciences, December, 1888.
[5] Internat. Encyclo. Surgery, 2d edition, vol. iv.

trunk without any support, thus increasing the risk of sharp flexion and deformity.

The present state of our knowledge justifies us in concluding:

1. Improvement from pressure paralysis in vertebral osteitis is common, and may be confidently expected when extension and fixation can be properly carried out, provided motion and sensation have not been entirely absent for many months, and when serious cord changes have not occurred. Hence an operation should not be undertaken until these measures have been persistently tried and proven ineffective.

2. Vertebrectomy for the relief of pressure in spinal caries is still most emphatically in its experimental stage.

3. The operation, even if effectual in relieving the pressure upon the cord by permitting posterior expansion, or from removal of bony or inflammatory growths, yet promises no certainty of permanent relief, since degeneration of the cord may already have been far advanced.

4. Early operation would doubtless be much more successful, as it might antedate these cord changes; but our present diagnostic facilities do not permit us to distinguish accurately these cases from those in which a less serious form of treatment will be followed by the rapid regain of lost power. Hence early operation is undesirable.

5. Pending further knowledge, we can say, however, that the operation is permissible where, after persistent treatment by suspension and fixation, complete motor and sensory paralysis, with spastic rigidity of the muscles, exists, together with exaggerated reflexes which have been present, but which have after a time subsided or been lost.[1]

[1] Glasgow Med. Journ., 1886, xxv. 210.

TREATMENT OF POTT'S PARALYSIS BY SUSPENSION, ETC.

By S. WEIR MITCHELL, M.D.,
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[Read March 6, 1889.]

I HAVE seen in the last ten years many cases of paralysis the secondary results of angular curvature of the spine, and twice I have met with these palsies where there was tenderness over two or more vertebræ and local elevation of temperature *without* any visible curvature or projection.

It is hardly necessary here to dwell upon the causes of these palsies. They are due to inflammatory thickening of the membranes, causing pressure, which leads to myelitis and local atrophic destruction in the areas so compressed.

More rarely—very rarely—the whole bony spinal structure is so damaged as through its collapse to lead to pinching of the cord. In either case there may be, finally, degenerative changes in the cord below the portion diseased by pressure. Owing to the fact that the front of the spine suffers most, motion is primarily impaired, and sensation slightly, or not at all, or only much later in the case. Excessive reflexes early announce the interference of nerve paths and the presence of irritative changes, and only cease to exist if (as is usual) degenerative alterations affect the regions below the seat of carious disease. Finally, sloughs may occur, and death from exhaustion.

In other cases to be found in every abode of incurables, the palsy, in whatever degree it be present, is a thing for life, and

by no means the finally curable matter which many assume it to be. The common history of mild cases is one of motor loss with only more or less numbness, and the usual excess of reflex activity.

For many years of my life, carried away by modern authority and example, I put all cases of Pott's paralysis at rest, used tonics and oil, and cauterized the back. In most instances this method answers. It is slow, but it suffices. The spinal curve left to itself solidifies—the palsy fades away. The treatment is wearisome and confining, but at last the patient gets up and puts on a brace.

After some years I met with two cases in succession which did not get well at all, and, about this time, I found one or two children who were sure to die of tubercle if left in bed. In despair I fell back upon the treatment by suspension advocated by my father, Prof. J. K. Mitchell, in 1826.[1]

As soon as I began again to use this simple means, I saw that it was admirably effective. It lets the patient get out of bed; it distinctly alters the pathological curve of the spine; it gives ease and relief to aches; it seems to act with more speed than other plans, and, when these have failed, it gives a new resource.

[1] Amer. Journ. Med. and Surg., Jan. 1826. Some of the earlier surgeons, as Glisson, used suspension from the head, even adding weights to the legs. Darwin, the author of "Zoönomia" (1801, p. 140), proposed extension by crutch supporters set on the arms of a chair with *occasional* aid by suspension from the head. He seems to have preferred suspension in bed on an inclined plane. M. Le Vacher, whom he quotes, in 1768 used suspension by the head, the weight being borne from the hips. Shaw (London, 1827, p. 98) treated his cases by the inclined plane. The cut in which he figures a suspension by the head (p. 107) refers to the use of a jacket (p. 32) as a support for the head-stay. Bamfield also seems to have used suspension, or mere sustentation, kept up from a jacket supported on the pelvis, Sheldrake's curious book (1798, p. 20, *et seq.*) figures a pelvic support for a head-suspending iron. Most of the attempts to suspend either used a jacket as a point of support, or employed the inclined plane. No reasonable systematic suspension, slowly increased, or combined with a chance to move about, preceded the date of my father's paper of January, 1826. Pott's paper bears the title of "Essays on Palsy of the Lower Limbs," and his account of this spastic paralysis has the distinctness of a master-hand. He speaks with some scorn of all machinery, and, as we all know, relied on issues, setons, and the like. Of late years Benjamin Lee, in an able paper, has recalled attention to the results and methods of J. K. Mitchell.

For the orthopædist of to-day the back is to be splinted. The conditions in Pott's disease are likened by him to those of a fracture; but if compared to any, it should be to one of the thigh where the muscles are continually pulling at the fragments. The problem is this: If we draw upon the bent spine of caries, so as slowly to alter its curve, will the gap we create be filled up by tissue firm enough for support? It seems to be so. So much for the mechanical part of the question. It is needless to go further as to a matter settled by numberless cases.

When, in addition, we come to ask how extension acts upon the spinal cord, and the inflammatory products which compress it, the question is more puzzling. Why is it that pulling at the curve, in some way before we much alter that curve mechanically, and often in a few days, so changes the condition *within* the canal as to allow sensory impressions to pass the seat of stricture?

If the spinal cord, in place of hanging in normal suspension in the canal, is in these cases of caries bound fast and anchored, so to speak, it seems probable that any serious extensive force exerted on the angulated column of bones must more or less pull upon and disturb the binding masses of inflammatory deposit. I believe, however, that there is something more than this involved, and that the pull made on the whole length of the bony column, even when no meningeal inflammation is present, may, in some way, alteratively affect both cord and membranes through stretching of the cord and alteration of vascular conditions. With this idea I have twice used suspension in cases of spastic paralysis in which there was no caries. Both proved failures. Meanwhile, in Russia and France (if we may trust a statement in the *Progrès Médical* of January 19, 1889) this means has had a brilliant victory in a fair share of cases of posterior sclerosis. I am now experimenting with it in this disease. I have no intention of entering fully on the general question of the therapeutic management of spinal curves caused by disease. I believe that, with rare exceptions, all cases of deformity of spine from caries are best treated by

early use of suspension, in conjunction with other means; but my present object is to show that Pott's palsy (as it was called) is also best dealt with by this plan.

As to the methods to be employed, something must be said. When in 1826 my father used suspension, he devised a number of ways of applying it so as to allow the child (for it was the young of whom chiefly he speaks) to sit up, to move in a spine-car on rollers, to ride a hobby-horse, or to sit in a swing. A curved iron, made movable and set by a screw, sustained the head-sling in which the head was held by the chin and occiput. His plan was cautious, and even deliberately slow, and as to it I leave the following quotation to speak:

"I have usually made very little extension for the first week; but have, thereafter, elevated the rod from one-eighth to one-fourth of an inch weekly, in proportion to the yielding of the spine. The progress of the reduction of the curve is, of course, more rapid at first; but as the curve is lessened, the mechanical advantage of the suspensory apparatus is equally diminished; and its effect is also resisted when that degree of curvature is removed which depended exclusively on causes having no relation to change of structure. But when a remnant of deformity is kept up by a change in the form of the vertebræ, and their *intermedium*, the operation of the remedies must be tedious as was the disease during its formation. When there is caries, we must wait for the production of new bone to support the wasted column; and when there is, without caries, a wedge-like condition of the vertebræ, we must rest on the slow, but certain, processes of absorption and deposition for a final cure. If undue pressure on one side of the column produces the deformity, the removal of the pressure to the other side must ultimately correct the distortion.

"For the fulfilment of the second indication nothing more seems necessary than to use the means suggested for the first. Cases *may* occur, of so violent and intractable a nature as to demand continual support. Such must be kept, during the whole period of treatment, in one or other of the many instruments designed for the suspension of the head, and which at the same time interfere not with either general or partial exercise.

"It is not necessary to exceed four hours a day of suspension, but patients often like the application of extension to last longer, and they pass six hours or more in the head-slings."

All of the means alluded to I have employed, but in adults and in the wards of the Infirmary for Nervous Diseases we

make use in many cases of a cross-bar placed over a bed or a chair, and from this suspend the patient. A good plan for the chair suspension is to use a low seat on which are placed a dozen or less of thin one-eighth or one-quarter inch pieces of board. By removing one of these from time to time systematic extension is obtained.

My father usually proceeded, as has been seen, by cautious degrees. I, however, have been accustomed, especially in the young, or in young adults, to make at first a strong pull on the spine by suspension, and to leave the patient's sensations to govern the amount endurable. In younger people I follow my father's rule of very gradual increase in the amount of pull made by suspension. As it has become interesting to know how much pull on the bent spine is well borne, I have lately put a little steel spring balance in the suspension apparatus, with these results as to endurance. Many of these diseased people bear the pull of twenty to seventy pounds for four to six hours a day. The pain, the sense of gain, the fatigue, are good guides for us. A great variety of suspensory means may be used according to the site of the trouble, but it is so far clear to me that suspension by the head, partial or complete, aided or unaided, should be employed in every case where palsy is one of the results of the caries.

In cases of spinal maladies, to get the alterative of complete suspension, the plan of Matchoukowsky may be followed, or the plaster jacket used with or without additional pull on the head.

It is interesting to watch a case of Pott's palsy subjected to partial suspension treatment. The patient becomes more and more at ease, and, if in a wheel-chair, can move to and fro while suspended. In ordinary cases, where the vertebræ have not become reconsolidated in their new position, and where there is only a relative degree of motor palsy, the change, as I have said, is often so sudden as to excite surprise. In older or worse cases, where sensation has suffered, or where the third degree has arrived—that is, degenerative change below the curve—in all of these we must look for very slow gains, and

welcome them first in the return of sensation, and then of the reflexes, from below upward.

Since writing this brief paper, I have read an editorial in *The Medical News* relative to Dr. Wood's method of making suspensions for the treatment of Pott's disease. It seems to me reasonable for cases of lower dorsal curvature alone, and I think it might be more so, if, while lifting from the jacket, a portion of the pull was made, also, directly through the head. I, myself, have never found anybody who would not bear this partial suspension from the head alone and for hours. The pull it makes from the head is a very direct one upon the spinal column; the pull made by Dr. Wood's jacket must distribute itself over considerable space, and much of it must fail to influence the spine. Thus in his method the amount of pull which really acts on the spine is impossible to determine, and must depend to a considerable extent upon the situation of the carious vertebræ.

I shall certainly in the future suspend with both head and jacket in lower and in even mid-dorsal curves, and I am now using the jacket in a case of extension for ataxia. In cervical caries extension from the head is imperative. In old consolidated spines with palsy not due to bony pinching of the cord, I would use both means of suspension, and daily prolong the amount of this treatment. In very bad early cases with abscesses, I should desire to use extension, at first, by the milder method of the inclined plane. As to the medical treatment of Pott's disease, it is needless to speak; we all agree as to that; and, of course, in every case of palsy electricity should be used to keep up the muscular nutrition. This may be aided by passive motions and mild effleurage of the skin; and, later, by deep kneading, never to last very long. Neither does suspension exclude the use of frequent cauterization, for which Charcot claims so much. I may add that I have seen suspension succeed where frequent use of the hot iron had failed. It is interesting to find that my father, who had twice been in the East, recommends shampooing, percussion, and friction for the children under treatment. It remains to defend my posi-

tion by relation of cases, and I shall as to this limit myself to the narration of three cases, but I shall also quote others from my father's essay.

This present paper does not pretend to bring forward any novelty; but, as I find neurologists all adhering to methods of treating the palsies of Pott's disease which appear to me ineffective or irrational, I have ventured to set forth anew a treatment which had so large a success in my father's hands, and has now again had it in mine.[1]

The following cases from his paper I quote, because his essay is not easily to be had, and because of the brilliant results he relates in cases of caries in various parts of the spine, and in instances of paraplegia following Pott's disease.

"I have seen no case of *posterior* curvature which did not cease to cause pain so soon as it was subjected to the action of the spine-cart. Irritation ceased, the tenderness of the part departed, and where the legs were affected they recovered their ordinary powers. In fine, to all appearances, there ensued a complete arrest of the diseased processes. The recent writers object decidedly to the use of suspension in carious cases, on the supposition that there exists considerable danger of fracturing the spine at the diseased part, even after ankylosis has supervened. After the occurrence of ankylosis there can be no good reason for using my instruments, unless the disease should extend itself further.[2] Where there is advantage to be derived from extension, I would use them, without much regard to such theoretical objections, because the objectors are not able to produce a single instance of such an accident arising from mechanical extension.[3]

"To demonstrate more clearly the utility of the plan of treatment proposed in this paper, I beg leave to describe a few cases.

"On the 1st of February, 1842, I was invited by an eminent physician to see with him a case of curved spine. A. S., a boy, about five years of age, was the subject of this disease. At that time his occiput rested on the *vertebra prominens* so as to prevent any portion of the neck from being visible from behind. The pressure of the head upon the space between the shoulders had caused painful excoriations. In endeavoring to cleanse the back of

[1] Gowers speaks of suspension as possibly advisable, especially in children, where other means have failed, where it is said to have been followed by instant improvement. He gives no authority.

[2] This is not the case, however, in curvature with paralysis. The curve may be old and unchangeable, but, nevertheless, the palsy will get well after use of suspension. —S. W. M.

[3] North American Medical and Surgical Journal, January, 1826, Dr. J. K. Mitchell.

the neck, it was necessary to insert a wetted cloth edgewise, as any attempt to introduce a finger gave considerable pain. Besides this posterior curvature which formed a very acute angle about the uppermost dorsal vertebra, there existed the serpentine or lateral curvature, so well described by Dr. Shaw. This curvature was convex toward the right shoulder above, and convex toward the left hip below, a direction of lateral curvature from which it seldom, if ever, deviates. At the angle caused by the *posterior* curvature the patient felt, at times, severe pain, particularly when in motion. Indeed, so painful was a rotary motion, especially in bed, that he found it necessary to call for the assistance of an attendant when desirous of changing his position. During the greater part of the day he sought refuge from pain in bed, or in the arms of his mother or his nurse. His pulse was rapid, without force or regularity. Exacerbation of febrile symptoms in the afternoon, and colliquative sweats during the night, afforded proof of a hectic state of his system. His appetite was irregular, his tongue encrusted with a moist, russet-colored substance, and his breath very fetid. His bowels were usually costive, but sometimes relaxed. At times he complained of severe pain in the epigastric region. His skin was, during the day, dry and rough, and was covered with bran-like spots, as if the cuticle was separated from it in scales. For a few days before the consultation, the child had lost in great measure the power of deglutition. The water which he endeavored to swallow generally escaped from the angle of his mouth.

"The remedies used during the previous treatment of his case were, laxatives occasionally, and afterward the chalybeates, and some other tonic medicines. The child, before it became too feeble to endure exercise, had been directed to swing by grasping a suspended bar of wood and supporting its body by means of its hands and arms. The exercise was found to be too violent, and could not be endured long enough to produce beneficial results.

"In consultation, February 29th, we resolved to try the effect of such suspension as would relieve the patient of a portion of his weight, and at the same time permit him to take exercise.

"The mechanical deformity was treated by the chair with rockers and the spine cart. After a short time the child refused to sit in the chair, but continued to use the cart four or five times a day. From the moment at which these means were applied there was exhibited a general amelioration of all these symptoms. The curve became less and less conspicuous—that which was lateral entirely disappeared—and there now remained of the angular projection only enough to show where the disease had been seated.

"On the 2d of June I deemed professional attendance no longer necessary, and to this day[1] the child remains robust, active, and cheerful, without deformity which would be observed by those who had not been acquainted with the previous history of the case.

[1] He is still alive and well.—S. W. M.

"Several other cases resembling the above in the angular and lateral curve, the impaired digestion, and roughened skin, have been treated with equal success. One of them, the case of E. F., fell into my hands on the 4th of September of the present year. The patient is, at the date of this paper, December of the same year, in good health, and rather more than two inches taller, in consequence of this straightening of the curve.

"One of the most difficult and intractable cases I have yet encountered was that of E. C., a girl, four years old. This child had a posterior curvature, seated nearly in the middle of the back, and a serpentine curve so great as to cause the lower limbs to repose on the crest of the os ilii of the right side. The projection of the shoulder and hip was such as to excite the notice and sympathy of every one. The child remained about four hours a day in the spine-cart, and was left at liberty during the remainder of the time. At present the dorsal prominence is small and apparently diminishing; and although the child still exhibits some lateral curvature *when it is off its guard*, it is able to carry itself, when it chooses, nearly in an upright manner. While *in* the spine-cart no curvature of the spine is perceptible.

"James H., a boy, aged about seven years, was brought to me in May, 1824. He had evidently a disease of the middle dorsal vertebræ, by which the bodies of two, at least, of the bones appeared to be affected. A shuffling gait, observed about six weeks before, led to the examination of the case, and a physician at New Castle discovered the curvature. In this case one of our most eminent physicians advised issues; but as the mother was predetermined not to use that mode of treatment, the child was brought to me to be placed in a spine-cart. One was accordingly obtained, but was not used regularly, because of the unwillingness of the boy to submit to any restraint. The progress of the complaint was rapid, notwithstanding the use of purges, baths, etc. At length the lower limbs began to fail in their office, and the poor boy was compelled very frequently to throw himself, for ease, on his face on the floor. When he ascended the stairs he was obliged to go on all fours. At this time he expressed greater willingness to endure the restraints of a machine, because he found himself more comfortable when in one. In a few days, strength and facility of movement returned, and since that period no increase of curvature has been perceived. His general health is now good, and he has a remarkably robust appearance. It may, in this place, be proper to remark that in every case, except one, in which these machines have been applied, either to posterior or lateral curves, the tone and vigor of the system have been immediately restored, and the pain and tenderness of the part concerned have disappeared."

The following cases are my own.

One, as yet incomplete, is interesting for various reasons. I merely give, at present, a summary of its notable facts.

Mr. T., aged thirty-six years, angular curve in mid-dorsal region.

Two years ago he first noticed paralysis of motion, in March, 1888. Sensation suffered some months later in both hands, and had been entirely lost for five months in November, 1888. There was also complete loss of reflexes below the navel. There were, also, rectal feebleness and vesical palsy. There was erythema of nates, but no sloughs. He had been ably treated by the usual means—rest, cautery, etc. His general state was good. Suspension was used in December, and gradually increased to two or three to four hours a day, and the pull from twenty to sixty-five pounds. In two weeks there was partial return of sensation, which slowly improved. There was also return of clonus. In five weeks there was very slight return of the power to move the second and third toes of the left foot. Since then there is very slow increase of sensation, which varies daily, as does the growing power to move the toes.

Of the future of this case I cannot speak confidently. It shows certainly the influence of extension on a spine already considerably consolidated, and I mention it here as being one of the worst I have ever tried to handle.

C. C., aged twelve years, of apparently healthy family, without tubercular history on either side.

In March, three years before I saw him. he began to complain of pain in his back, where, on examination over the middle dorsal region, there was found a slight projection. The parents of the boy being in the lower ranks of life, little attention was paid to his complaints, but, after six months, the curvature of the spine and the increasing amount of pain rendered it necessary for a physician to be seen. Under the advice of this gentleman, the child was given an ordinary spinal brace, which, however, did not appear to answer any better purpose than that of irritation. At this time there was more or less fever in the evenings, and a great deal of pain running round the flanks from the seat of the disease.

Late in the year there was more or less numbness in both extremities. In the fall the patient was placed in bed, where he remained all the winter; in fact, most of the time since, up to the date of my attendance—that is to say, for two and one-half years. During this long repose the curvature of the spine increased, the pain grew greater, and the limbs became numb. The patient was completely insensible to touch or to pain, and very soon after there arose, also, an increasing difficulty in motion.

When first I saw the lad, two years and a half from the period at which the disease began, this was his condition: The disease seemed to have followed the ordinary course of spinal-pressure paralysis: First, pain, loss of sensation in all of its kinds, then difficulty in motion, and, finally, loss of motility.

The reflex excitability went, however, far beyond the ordinary expression. A light blow with a percussion hammer on the shin caused the leg to fly up a foot or two from the bed. When six months later I saw him again, there was a marked degree of degenerative change in the muscles below the waist, and there was neither sensation, motion, nor reflexes. Water accumulated in the bladder, and had to be drawn off from time to time. The condition of the rectum was such that the feces had to be dug out, and the state of the child was most pitiable. The only thing which he lacked to complete the misery of this picture was the presence of sloughs, such as existed in one other case to which I shall call your attention.

So far as treatment is concerned, he had had cauterization, long rest in bed, with efforts to extend him while there, and a plaster jacket, which, so far as I could learn, had been very well put on. None of these means had done more than to make him comfortable by the relief from pain.

It was evident that in this case there had been pressure from hypertrophic thickening of the dura, since the curve was not sharp enough to have punched the cord. But, also, it was clear that there had been a degenerative change below the pressure point, because the reflexes had entirely disappeared, and because the muscular responses to a blow were feeble or absent. Certainly, there was little to encourage treatment.

I directed the parents of this boy to have made a wooden support from which was hung a cord, to which was attached an iron arrangement according to the figures in my father's paper of 1826. It was found, after two or three days, when the child was sitting under this support with his head lifted and fully extended, that the pain did not trouble him, although he had never before been free from pain when sitting upright since he was attacked by the disease. This fact made him very willing to undergo the discipline necessary, and his parents were enabled in a few days to increase the amount of suspension to two hours morning and evening. No attempt was made to measure the suspensory pull, of which, however, I shall presently speak in regard to another case. The amount of lift exerted by this little apparatus was simply that which was comfortable, and never went so far as the whole weight of the body. The result was most striking. Within ten days after the suspension began there was a considerable return of active pain in the legs, together with the positive sense of touch over a large part of the limbs. Three days later he could tell which toe was touched, and he had generally good locality impressions as to touch within two or three inches of the place selected for trial. The pain sense came back nearly at the same time, and it then seemed to me to be extraordinarily acute.

At this period, also, there arose certain alterations in the nutritive state of the limb. This seemed decidedly apparent to his parents and friends. The

skin became less dry. The brawn-like hardness which I had seen about his ankles passed away very rapidly, and the general rigidity of the muscles lessened. It was fully a month after the suspension began before he recovered any power over his bladder. The return of capacity to empty the rectum was much slower; it had to be stimulated by electrical treatment. Thirty or forty days after the suspension began, motion could be made in the right great toe; from this motility spread over the whole right limb and then began in the left.

It is hardly necessary to trace this case further; it is only needful to say that there was gradual return of all the functions, so that at the end of four months I was willing to permit the lad to get upon crutches and to wear a plaster jacket, but for fully eight months he passed a certain length of time in the suspensory chair, or in a spinal car, somewhat similar in appearance to the one described in my father's paper.

This lad made a fair recovery—that is to say, there is now no loss of sensation in the limbs—and he is able to walk about with the aid of a stick. He is capable of earning his own living.

A. B. C., a physician, aged thirty-two, and unmarried. He comes of a family noted for health and long life; free from scrofulous or tubercular disease, and without history of cancer nearer than four generations.

Previous to 1883 the patient's health was excellent. Aside from the slight ailments of childhood he had been perfectly healthy; never had venereal disease. In July, 1883, while playing football, he fell, striking hard upon the back, others falling upon him. Notwithstanding the severe concussion, he was able, in a few minutes, to walk home, and recovery from the immediate effects of the accident took place quickly.

An urgent country practice called this gentleman out upon long drives over rough roads, and, after some weeks, pain was felt in the right flank. This was occasioned by severe exertion or sudden jolt. No attention was paid to this at first; but the pain increased in severity and constancy until November, 1883, when driving had to be given up. The pain was confined to the right side, and was not felt in the back. There was no prominence of any vertebræ; no weakness of the limbs. Meanwhile the general health was good.

The use of the actual cautery once a week, and small doses of iodide of potash, colchicum, and guaiacum gave no relief; on the contrary, the pain and soreness grew worse, extending to the back.

In January, 1884, weakness of the lower limbs was noticed. This came on when walking up hill or walking fast. The patient's legs began to twitch and jerk, and he had a tonic spasm of the muscles of the legs on awakening in the morning. The knee-jerk was exaggerated at this time and so remained.

In March, 1884, a prominence of one of the vertebræ was observed, and the diagnosis of Pott's disease was made. A plaster-of-Paris jacket, the actual cautery, the bichloride of mercury, iodides, and tincture of iron were used without avail, and in one month paraplegia was complete. Sensation

was also abolished. The sphincters were powerless; the urine became alkaline and loaded with phosphates; priapism was present occasionally. The legs were jerked about in almost constant spasms. The feet, when at liberty, were drawn up to the body, and had to be tied fast to the bed. The legs were greatly wasted. Soreness in the back and sides continued to be severe, and for only a few minutes at a time could the patient sit up in bed.

One year later, in very much the same condition, but with sloughs of the nates, he came under my care at the Infirmary for Nervous Diseases.

The treatment adopted was at first the prone position and application of bags of ice to the spine. After ten days, hot water and ice were applied alternately to the spine four times daily for about half an hour each time. The bed-sores were treated with ice and hot water, and easily healed. At the end of five weeks of this treatment the first changes were noticed. The spasms in the legs diminished somewhat. Sensation was still absent; but in the third week of treatment, at first touch, and then pain and temperature sense began to be slightly noticed in the legs above the ankles. The sensory changes were trivial, and, after five weeks in the infirmary without increase of betterment, the patient was sent home, and a plan of treatment adopted to which his cure is due.

Two weeks after his arrival at home (there having been no further change under ice and heat) a system of spinal extension was used. To the back of a wheel-chair was attached a steel shaft, curved so that it extended over the head. A head-piece, similar to that used in extending patients in applying plaster-jackets, was attached to this shaft. At first only a moderate pull was made, as it was somewhat uncomfortable, causing slight pain at the curve of the back and sides, and in the neck. But the patient soon became used to it, and was finally, at times, suspended by the whole weight of the body—120 pounds. No injury was done by this.

The back of the chair in which the patient sat was elevated many times a day, so that he was in a sitting position with a steady extension of the spine. Care was always taken that the back of the chair was lowered to a reclining position before the head-piece was taken off. On going back to bed the head-piece was attached to a pulley over the bed, and thorough extension was used before lying down.

Hot water and ice were used every day, and faradic electricity to the paralyzed muscles. Improvement came by degrees and sensation improved continuously. In three months the patient could move one toe a very little; in a few months he could stand, and in a year he could walk about. As motion improved, power over the sphincter returned to full strength. Sexual power had been lost, but now was restored. As motor gain took place sensation also improved, and is now normal. The muscular spasms which had characterized the case gradually lessened, and their lessening gave some of the first gleams of hope in this dark experience.

The patient writes that he is still gaining strength and gradually resuming

his accustomed work. Suspension has been given up by degrees; but still, if the back aches after being long afoot, he uses suspension for a little while as a relief. A spinal brace put on nearly a year ago, is still worn for ease and support.

The curve at the seat of the disease in the spinal column is considerably less than it was to begin with; the knee-jerk is still excessive—six and a half inches of foot leap, and there is a slight ankle-clonus, equal on both sides. However, as the man can walk several miles, is practising his profession without pain, with all his bodily functions restored, I look upon his cure as one of the greatest triumphs it has been my fortune to witness.

My conclusions are—

That suspension should be used early in Pott's disease.

That used with care, it enables us slowly to prevent or to lessen the curve.

That in these cases there must be in some form a replacement of the crumbled tissues.

That unless there is great loss of power, the use of the spine-car or chair, etc., of John K. Mitchell, enables suspension, especially in children, to be combined with some exercise.

That no case of Pott's paralysis ought to be considered desperate without its trial.

That suspension has succeeded after failures of other accepted methods.

That the pull probably acts more or less directly on the cord itself, *and that the gain is not explicable merely by obvious effects on the angular bony curve.*

That the now well-known influence of extension in Pott's palsy makes it probable that in other forms of spinal disease, not due to caries, extension in various forms may be of value, as has apparently been of late made clear.

That the methods of extension to be used in these and in carious cases may be very various, only provided we get active extension.

That the plan and the length of time of extension must be made to conform to the needs, endurance, and sensation of the individual cases.

DISCUSSION.

Dr. W. W. Keen: I have but a few words to say in reference to this subject. I have not operated on the spinal column in the living, but I have made some experiments on the cadaver, and have been struck with the facility with which the spinal cord can be exposed and operated upon. In these experiments I have generally removed the laminæ, either by trephining on either side of the spinous process, or, as advised by Mr. Horsley, by cutting off the spinous process and then trephining in the centre. Some three weeks ago I went to New York to see Dr. Abbe operate on the spinal cord, and I was struck by the large amount of room obtained by removing only one-half of the laminæ. This was a case in which he removed the posterior roots of several of the cervical and of the first dorsal nerves for obstinate neuralgia, which had already been treated by resection of the ulnar nerve and stretching of the brachial plexus. He easily obtained ample room for the removal of the posterior roots. I also noted that he used only the rongeur forceps in place of the trephine, and removed the laminæ without difficulty. The advantage of working from below upward, on account of the overlapping of the laminæ from above downward, was also apparent. It is to be remembered that in these cases, as in that of Horsley, the spinal nerves originate higher up than the point indicated by the symptoms. In Horsley's case the tumor was four inches above the point indicated by the anæsthesia of the surface.

In my experiments I have found no difficulty in exposing the posterior roots, and, by elevating the cord with an aneurism needle, in examining the anterior roots, and even the anterior surface of the cord, the anterior part of the theca, and through it the posterior surface of the vertebræ. In this way we have perfect access to the anterior as well as the posterior surface of the cord.

I have a word to say in regard to suspension from the head. In my lectures at the Academy of Fine Arts I am obliged to have my subject suspended on account of the mechanical arrangement of the room. The ordinary method of suspension is by making a hole on each side of the skull and inserting hooks into these holes. On one occasion a male subject weighing about 275 pounds was delivered to the Academy for my lecture. I had a great deal of doubt whether or not the skull and the tissues of the neck would sustain this heavy weight. There was, however, not the slightest difficulty, and the subject passed through repeated manipulations without the least detriment. This shows how great a weight the cervical tissues, and especially the cervical vertebræ and ligaments, will bear.

Dr. H. C. Wood: I suppose that it is hardly necessary for me to say anything in regard to an editorial which may have appeared in *The News*, based upon a more or less complete report of one of my lectures. This matter has possibly excited sufficient attention for me to say a few words in regard to

the plan of extension which I have employed at the University Hospital and the results obtained. The method consists in putting bands in the ordinary plaster-jacket, so that the patient can be hung from that jacket, using the upper truncated cone of the body as the resistance for suspension, and allowing the weight to be taken on various points in the chest. I have found that by means of this jacket it is possible to maintain a steady pull of from eighty to one hundred pounds for hours together without inconvenience to the patient and without previous training. I have also found that it is better to add to it suspension of the head, care being taken that but little strain is put upon the head. In this way the patient may be suspended not only for one, two, or three hours, but extension may be maintained for twenty-four hours. All that is necessary is to elevate the head of the bed and fasten the jacket to it. I have in this way kept a patient at an angle of forty-five degrees for days together.

In a case of syphilitic tumor producing pressure upon the cord, and also caries, this treatment was employed. The patient had been under treatment for months without improvement; he was unable to lift his feet from the bed at all. Inside of two or three days after the application of the treatment he was able to move about freely, and in about two weeks could walk without the aid of a cane. The gain has been steadily maintained. There was distinct angular curvature. The antisyphilitic treatment was continued.

I think that the gain is accelerated by changing the spinal jacket once in ten days or two weeks, perhaps a little oftener. The new jacket should be put on after the patient has been suspended for two or three hours, in order to get the greatest spinal extension.

I have had no case of caries of the neck in children, but I believe that the spinal jacket can be used there; under these circumstances, not as a means of suspension, but as an abutment against which extension can be made. By using a jury-mast with a screw, the extension can be regulated without hanging the patient in a chair.

Heubner affirms that there is local tenderness on deep pressure in spinal syphilis. I have seen many cases of spinal syphilis without tenderness, and in every case in which there was tenderness, disease of the bone has shown itself. At the autopsy of such a case I have found a tumor in the membranes, on the one hand involving the cord, and, on the other, pressing upon the vertebral bodies. I am strongly inclined to believe, that where in spinal syphilis there is distinct vertebral tenderness on jarring or deep pressure, that you have not merely the cord involved, but also that the bone is affected.

Dr. Mitchell: I think that this method with the jury-mast and jacket is similar to that of Sayre. It is a good plan, but not so good as suspension. Suspension is the best of all in these cervical cases. One of the most brilliant results recorded in my paper is that of a gentleman still living in this city, who, when a child, was treated by my father for a most horrible form of cervical caries. Dr. Wood and I are quite at one. We want to bring into

prominence what has been thrown into the background, by reviving this method of extension in its various forms. With the additions of modern science we can better hope to relieve these conditions than could the older surgeons.

There is only one other point. I am surprised that none of the speakers has referred to local thermometry in these cases. I cannot help thinking that if, in some of these cases, careful observations of the local temperature had been made, the increase of local heat would have indicated the proper point for operation.

Dr. Wood: My idea is not simply to use the jury-mast in the way that Sayre does, but with an apparatus by which you can regulate to a pound the amount of extension. There is, however, no reason why suspension by the head should not be used during part of the time.

Dr. Deaver: The manner of exposing the spinal cord I think depends largely upon the tastes of the operator. In the case of Dr. Lloyd and myself the exposure was made by removing the spinous processes with bone forceps and cutting through the laminæ. The great tendency of to-day is to a multiplicity of instruments and complicated ones. In my opinion, the simpler an operation, the more effective it is.

Dr. Willard speaks of the use of the aspirator for the evacuation of pus. I should look upon the aspirator simply as an instrument for diagnosis, and not for treatment. After the diagnosis is made in these cases of chronic abscess, then is the time to open them by dissection and treat them antiseptically, and not to wait and lose valuable time.

HEADACHE ASSOCIATED WITH UNUSUAL VISUAL PHENOMENA.

By G. E. de SCHWEINITZ, M.D.,
OPHTHALMIC SURGEON TO THE PHILADELPHIA AND CHILDREN'S HOSPITALS;
OPHTHALMOLOGIST TO THE INFIRMARY FOR NERVOUS DISEASES.

[Read April 3, 1889.]

In June, 1877, Dr. Weir Mitchell presented a communication to the Fellows of the College, in which he described four examples of a peculiar variety of migraine associated with apparitions of an unusual character. In these cases the ordinary zigzag lines and rotating wheels which have been so commonly reported in connection with hemicrania, were replaced by definite shapes so real that in some instances the belief was induced on the part of the patient that a ghost had been seen. In two of the persons described by Dr. Mitchell the vision came as the only prodrome of a severe headache. I will briefly recapitulate the chief points, as far as the apparitions are concerned, in Dr. Mitchell's cases.

In the first instance, a woman, aged thirty, had attacks of migraine of great severity; when nineteen years old, before the headaches, a bright, gold-tinted cloud appeared; nine years later, the vision which preceded the headache consisted of a large black dog, which usually appeared as the patient was going up stairs, and was so real that once she put forth her hand to touch it. In the second case, a woman, aged thirty-eight, the apparition which preceded the headache consisted in the appearance of a sister who had long been dead, dressed for the street. In the fourth case, a man, aged thirty, saw a variety of visions; complex and geometrical figures of brilliant pink

or red, multiple red circles in rapid rotation, and once a red eye, which apparently came from the distance; on one occasion a crescent of silver on the wall, and suspended from it numerous heads in profile, some were strange to him, and some were the revivals of faces which had long been forgotten. In one attack a red spider appeared, which merged into a series of red rectangles revolving in swift motion.

Since this communication I have examined many cases of severe headache, with the purpose of elucidating the presence or not of apparitions as prodromal or associated symptoms. More than this, I have searched very industriously through the literature in order to ascertain whether similar cases are upon record, a search in which I was aided by Drs. Fletcher and Lorini, of the Army Medical Museum. It is the result of this investigation which I desire to present to the Fellows of the College this evening.

Two of the cases sought relief in the service of Dr. Mitchell at the Infirmary for Nervous Diseases, and, so far as I know, are the only instances which have appeared in that clinic, which is so singularly rich in all the phases of nervous disease. Two others I use with the permission of Dr. Morris J. Lewis, and are from his private practice. He has kindly permitted me to examine them. One of the cases is from the private practice of Dr. F. X. Dercum, who permits me to use the case, as he also accorded me the opportunity of examining the patient. Finally, the last case is one from my own practice—a patient that I have had under observation for more than a year, and who also has on several occasions been seen by me with Dr. Lewis.

Case I.—Sarah W., aged forty-four, married, has seven living children, three dead, and has had five miscarriages. The youngest child is now five years old. The patient had scarlet fever and smallpox when eight years of age, but since then no other severe illness. When a girl, she was struck on the left side of the head with a stone. The family history is good, so far as this is obtainable. Eighteen years ago, after severe exposure to the sun, she had an attack of pain in the head, associated with vomiting, purging, etc., which lasted for a week. Since this time up to the present day, at intervals, she has suffered with severe head-pains. For the past year these pains have

been worse, coming on once a week, as follows: The pain begins at the supratrochlear point of the left side, crosses the brow, and goes to the occiput, or begins on the left side of the postcervical region and runs forward, the character of the pain being that of a succession of severe electric shocks of the most violent type, lasting sometimes twenty-four to thirty-six hours. After the headache has reached a certain grade of severity, visions appear. Figures of all sizes and shapes, startling and horrible in their reality, or, to use the language of the woman herself, "I see all manner of ugly kind of varmint, I call them, such shapes as I believe no one ever really did see."

Under treatment, which consisted of cannabis indica, antipyrine, and the use of a boracic acid injection for a leucorrhœa from which she suffered, some modification of the head-pains resulted; at all events, for several months the visions disappeared.

In January of this year a severe paroxysm of head-pain again occurred, this time associated with a large green snake as the only visual apparition.

An examination of the eyes of this patient has revealed healthy, oval, optic discs, hypermetropic refraction, and slight insufficiency of the internal recti muscles. Correcting glasses were given; ascending doses of nux vomica were ordered, together with a generous diet, and bromide of lithium during the paroxysms. Later, owing to her anæmic condition (although a blood count revealed 1 to 600,000, hæmoglobin eighty per cent.), iron was ordered. The last record, in February of this year, states the headaches are better. Two weeks have elapsed without a severe paroxysm, and since the appearance of the green snake, no new visions have been seen.

Case II.—Miss Susan B., aged forty-eight, applied for treatment in the Infirmary for Nervous Diseases, and gave the following history: Has long been troubled with dyspepsia and malaria. Eight years ago had an attack of intermittent fever, which now is most manifest in the spring and fall. For the last two months has had a dull aching rheumatic pain in the bones of the right arm, in the right knee-joint, and in the right wrist. Severe headache, which formerly appeared once in two months, now comes on every other day, the occiput being the situation of the most intense pain. This headache is ushered in by the following visual phenomena: The appearance of a rainbow or a rainbow light in the eyes, which is relieved by shutting the eyes. Zigzag lines, seen also in the dark, for the headache not infrequently comes on during the night. Definite shapes of animals, cats, and dogs, and quite usually the faces of people, both homely and comely, always unfamiliar, and always with headache.

A very usual order has been, first, a chill; second, fever; third, the visions and the headache, and fourth, though rarely, vomiting.

Physical examination revealed a normal heart and some increased splenic dulness. Full doses of quinine and hydrobromic acid produced slight amelioration, but did not stop the headache or the chills. She was then ordered the oil of eucalyptus, five minims six times a day. From the time that this

remedy was exhibited, the headaches, and with them the visions, absolutely disappeared, although once after its administration a slight chill occurred. Acute rheumatism of the wrist and knee developed, the pain of which was modified by the use of salicylate of soda, but there was no return of the headache.

An examination of the ocular conditions revealed slight, simple, hypermetropic astigmatism with two degrees of insufficiency of the external recti at five meters. The pupils facing a bright light were unequal, the right being the larger; their reactions were normal. The anterior chambers were shallow. The tension was normal. Each optic disc was a vertical oval, somewhat gray in the deeper layers, physiologically cupped, and the deeper edges veiled by hazy retina. There was no change in the fields of vision.

Case III.—W. L., unmarried, a patient of Dr. Morris J. Lewis, in reasonable general health, gives the following history of her headaches and of the visual disturbances which preceded them: Up to sixteen years of age she was free from any form of headache. Then headaches without visual hallucinations began, usually coming on every day. At this time for a number of months her eyes were inflamed, probably with granular conjunctivitis.

Exactly at what time the visions appeared as prodromes of the headaches, the patient is unable to state accurately. The pain now begins above the right eyebrow, or in the back of the head. Often preceding the attack gold rings appear, while linked silver rings are constantly present. Once, and this quite recently, the vision which preceded the headache was a band of Indians seated in a wagon. Not infrequently the appearances take on, to quote the language of the patient, "faces of men and women hideous enough to make me vomit."

In this particular case the eyes revealed the results of former granular lids. In the left eye, where the fundus was visible, there was an oval optic disc, gray in its deeper layers, with a broadened scleral ring below, and a low degree of simple, hypermetropic astigmatism. In the right eye the cornea was too hazy to admit of perfect ophthalmoscopic examination. A slight convergent squint was manifest, and also ptosis, or rather a droop in the lids, the result of a former trachoma. These headaches were never in any apparent association with eye-strain.

Case IV.—Andrew L., aged fifty-three years, a patient of Dr. Morris J. Lewis, a night watchman by occupation. He consulted Dr. Lewis for relief from vertigo, violent headaches, unusual visual phenomena, and nausea. Those headaches were both ushered in and associated with peculiar and striking visions which, for the most part, took on the form of baskets of fruit, like oranges and gaudily colored flowers.

His eyesight was very defective, owing to a high grade of hypermetropia, + 8 D. in each eye, the correction of which did not yield him more than one-fifth of normal vision. The optic discs on each side were oval; a web-like haziness surrounded the margins of the discs, especially above and below, and

both arterial and venous lymph sheaths were full. The field of vision for form was not changed; that for colors was not taken.

CASE V.—Mrs. H. B., aged thirty-seven years, a patient of Dr. F. X. Dercum. Since early childhood this lady has suffered from violent type of "sick headaches." About the time that she was married, or shortly afterward, a very constant prodrome of the attacks of head-pain was a singularly real apparition, first usually in the form of her servant, or some other known member of the household, which appeared just at her left shoulder. In a few moments after the appearance of the spectral form, violent headache began, generally in the temple and upon one side, followed by nausea, and sometimes by vomiting. At present, the vision which most frequently appears is that of the forms of her own children, of whom she has had six, so real that on many occasions she has put forth her hand to touch them.

Two years ago Mrs. B. had pneumonia, and she is now the subject of uterine disorder. The eyes are entirely healthy; the external eye muscles normal, and both direct and indirect vision natural. Treatment in this case has, I think, not very much modified the headaches or the vivid apparitions which are their prodromes.

CASE VI.—Miss Emily Z., a patient of my own, sent to me by Dr. Lewis for the treatment of aggravated headaches in February of 1888. Before 1887 she never suffered from severe headaches, but frequent saw rising and falling silver balls and bright stripes, especially if looking intently at any near work. After 1887, during the summer, headache began, situated in the right parietal eminence, and gradually creeping backward to the occiput, associated with a drawing backward of the head, and golden balls flying everywhere through the air.

The patient had reasonably healthy optic discs, slight insufficiency of the internal recti, and well-marked, compound hypermetropic astigmatism. This was corrected under a mydriatic, the glass proving to be

$$\text{O. D.} + 0.50^{s} + 1.50^{c} \text{ axis } 90^{\circ} \frac{15}{\text{XV}}.$$

$$\text{O. S.} + 0.50^{s} + 1.^{c} \text{ axis } 90^{\circ} \frac{15}{\text{XV}}.$$

This she wore constantly; took iodide and bromide of potassium, and locally employed a weak solution of the sulphate of eserine in her eyes. Under this treatment for many months the headaches were distinctly alleviated.

In December of 1888 she returned, stating that for three months the headaches had reappeared. The rising and falling silver balls and the flying golden balls in the air were still present. Now, also, marked visions of animals, always either mice or dogs, were associated with the headache, which, however, was not more intense than on former occasions. Apparently the objects, so she stated, were seen only with the right eye. Once, in the morning, the vision took on the form of the water-pitcher in the room,

through the handle of which a constant stream of running mice playfully chased each other. Once, while sitting in a church, the vision was that of a small black dog that curled himself at her feet. On these, as on former, occasions the headache was present, but not more pronounced than when unassociated with visual apparitions.

The patient is somewhat anæmic; suffers from chronic constipation; has an obstinate leucorrhœa, and is nervous in her motions and speech, but not hysterical. Her family history appears to be good so far as general health is concerned; the eye history is the following: The father's sight is normal; the mother has progressive myopia, and is now nearly blind; two sisters are myopic.

A field of vision taken during one of the headaches revealed little or no change in the extent of the form field, but marked concentric contraction of the color fields.

Dr. Mitchell, at the close of his communication, remarks, "Comment in regard to these cases is difficult. The phenomena are not uncommon in disease, but their association as part of the complex symptom hemicrania is undescribed." This statement is very completely borne out by an exhaustive search of the literature, which resulted in a failure to find the records of exactly similar cases.

It is evident, however, that occasionally cases are on record of a somewhat analogous nature, generally to be found among the many records of spectral illusions with which the writings and reports of long ago are richly endowed. In Brewster's *Letters on Natural Magic*, a case is quoted, as mentioned by Dr. Abercrombie, of a gentleman who was a patient of his, of an irritable habit, and liable to a variety of uneasy sensations in his head, who saw one day a female figure enter wrapped in a mantle, with the face concealed by a large black bonnet. He knew that the figure was an illusion of vision, amused himself for some time watching it, observing as he did so that he could see through the figure and perceive the objects behind it.

From the *Philosophy of Sleep*, by Robert McNish, Chapter XV., "Spectral Illusions," pp. 243–277, I quote the following:

"Mr. Simpson, in the second volume of the *Phrenological Journal*, has published a case of spectral illusions which, for singularity and interest, equals anything of the same kind which has heretofore been recorded. The

subject of it was a young lady under twenty years of age, of good family, well educated, free from any superstitious fears, in perfect bodily health, and of sound mind. She was early subject to occasional attacks of such illusions, and the first she remembered was that of a carpet which descended in the air to her, then vanished away. After an interval of some years she began to see human figures in her room as she lay wide awake in bed. These figures were whitish, or rather gray, and transparent like cobweb, and generally above the size of life At this time she had acute headaches, very singularly confined to one small spot of the head. On being asked to indicate the spot, she touched, with her forefinger and thumb, each side of the root of the nose, the commencement of the eyebrows, and the spot immediately over the top of the nose. . . . . On being asked if the pain was confined to these spots, she answered that some time afterward it extended to the right and left, along the eyebrows and a little above them, and completely around the eyes, which felt as if they would burst from their sockets."

The account then goes on to describe in further detail the spectral illusions to which this patient was subjected, the variations which took place in them, and the fact that for a period of two years Miss S. was free from her frontal headaches and untroubled by visions or any other illusive perceptions. After a time, however, all her old distressing symptoms returned in great aggravation, associated with even more severe pain along the frontal bone and around the eyeballs. The apparition consisted frequently of single figures of absent and deceased friends which she met upon the streets, but which proved phantoms when she approached to speak to them.

From the same book I extract the following:

"In the fifth volume of the *Phrenological Journal,* p. 319, a case is mentioned where the patient was tortured with horrid faces glaring at her, and approaching close to her in every possible aggravation of horror. She was making a tedious recovery in child-bed when these symptoms troubled her. Besides the forms, which were of natural color, though often bloody, she was perplexed by their variation in size, from colossal to minute. She saw also entire human figures, but they were always as minute as pins, or even pinheads, and described the pain which accompanied her illusions, viz., acute pain in the upper part or root of the nose, and all along the eyebrows."

In the same volume other cases—as, for instance, that of Dr. Bostock, where spectral illusions were a prominent feature—are described, but they are even less relevant than the instances

which I have just quoted, belonging rather to that class of cases where apparitions form part of the phenomena making up a case of general illness, and are not prodromal or associated symptoms of hemicrania or any other type of neuralgic headache.

It should be remembered that the instances which Dr. Mitchell has recorded, and to which I have added these six cases, are peculiar in that the visions were "part of the complex symptom hemicrania." They must, then, not be confounded with similar phenomena in association with disease, nor with the visions that have been seen by a number of sane persons; "the Number-forms"; the association in some people of color with sound; the phantasy of certain individuals to connect visualized pictures with words; and that form of vision which is a phantasmagoria, or the appearance of a crowd of phantoms hurrying past like men in a street, it may be in broad daylight or more frequently in the dark—visions which have been seen and described by men eminent in the scientific and literary world.[1]

It is not a little singular that cases such as Dr. Mitchell and I have described have been so seldom recorded. Two explanations of this are evident; either that the instances are very rare, or that the patients thus afflicted suppress that particular portion of their symptoms in which spectral illusions play a part, when detailing the histories of their cases to their physicians —suppress them for fear of bringing upon their heads the unbelief with which anything that pertains to what may seem for the time to be supernatural is so frequently met. Moreover, as one of these patients once said to me, "I never told anybody about this because I was ashamed;" ashamed, I presume, lest the person, physician or otherwise, to whom the statement would be made, might have seen fit to scoff at the unfortunate sufferer. As Dr. Mitchell has pointed out, it seems difficult to find any explanation for these rare cases of

[1] Those interested in this subject will find a chapter of great interest, in which the varieties of visions seen by sane people just referred to may be studied in detail, in *Inquiries into Human Faculty and Its Development*, by Francis Galton, F.R.S., 1883.

headache preceded by hallucinations, just as it is impossible to find an adequate explanation of other spectral illusions in regard to which the accuracy of observation cannot well be called in question. In this connection let me quote from Brewster, who says:

"Although it is not probable that we shall ever be able to understand the actual manner in which a person of sound mind beholds spectral apparitions in the broad light of day, yet we may arrive at such a degree of knowledge on the subject as to satisfy rational curiosity, and to strip the phenomena of every attribute of the marvellous. Even the vision of natural objects presents to us insurmountable difficulties if we seek to understand the precise part which the mind performs in perceiving them; but the philosopher considers that he has given a satisfactory explanation of vision when he demonstrates that distinct pictures of external objects are painted on the retina, and that this membrane communicates with the brain by means of nerves of the same substance as itself, and of which it is merely an expansion. Here we reach the gulf which human intelligence cannot pass; and if the presumptuous mind of man shall dare to extend its speculations further, it will do it only to evince its incapacity and mortify its pride."

It is manifest from a study of the cases recorded by Dr. Mitchell and by myself, and also from those of somewhat analogous nature which have been gathered from literature, that this form of headache with unusual visual phenomena may be arranged into three groups: First, those varieties—and these are the most unusual—in which the apparition comes as the only prodrome of a violent headache of a neuralgic type. Second, those varieties in which the apparitions appear after the headache has been present for some time. Cases of this kind, I believe, more exact inquiry would prove to be more frequently present than the records seem to show. These are types analogous to the not very uncommon association of spectral illusions and various unusual visual phenomena in other forms of disease. Third, those varieties—and these may be considered as a subdivision of the class just quoted—in which the apparition appears at the end of the headache, and is, in short, the last scene in a more or less violent nerve-storm.

Finally, it is interesting to remember that visions have occasionally been seen by patients just before the development of a

lateral hemianopsia in that portion of the field of vision which afterward became totally and permanently dark. Thus Seguin has recorded a case in which chickens and other animals were seen by the patient who subsequently developed a complete homonymous hemianopsia, and I have myself seen and recorded a case in which the statement of the patient was that he had seen visions, chairs, tables, and other articles of furniture not really present, in the fields which afterward were obliterated. At the time I examined him the hemianopsia was already complete. Such phenomena must be explained by temporary congestions in the highest visual centres, the function of which is later to be entirely destroyed by a permanent lesion.

DISCUSSION.

Dr. Morris J. Lewis: I have been much interested in the paper, and I agree with the author that these hallucinations, associated with neuralgic headaches, must be more common than is usually supposed. Since furnishing Dr. de Schweinitz with the two cases he has reported, I have had a third, in a young healthy married woman, the subject, at intervals, of headache and supra-orbital pain for two years. With several of the headaches she has had the vision of a large cockroach, which was so distinct that she had to satisfy herself by investigation that it was not real; this vision always accompanied and did not precede the headache—never appeared without it. This case has been relieved by antipyrine. The patients were at first ashamed to acknowledge the existence of these hallucinations, and I believe that if these cases were investigated more thoroughly this association of symptoms would probably be found to be not uncommon.

Dr. Charles K. Mills: I have seen, in connection with migraine, several cases similar to those described by Dr. de Schweinitz. The proper explanation will probably be found in a study of the questions connected with visual localization. While making a collection of a large number of cases illustrative of various points in localization, I found twenty to thirty cases of hallucinations associated with lesions which were discovered post-mortem. A few of these cases had, in association with other phenomena, hallucinations of the same type as those described to-night. It is also well known that in epilepsy there are frequently visual phenomena closely allied to those referred to by the reader of the paper. Many of these cases have been described by Charcot, Richet, and others. These hallucinations are perhaps not so common in

ordinary epilepsy as in so-called hystero-epilepsy. It will be remembered that at least two or three cases of operation, guided by localization, have been performed in cases with visual phenomena somewhat of this character. A patient of Bennett and Gould, for instance, had peculiar hallucinations of sight, with epileptic seizures. The trephine was applied over the visual area, and although no gross lesion was found, after some excision or breaking up of the cortex, the patient recovered. The case of Macewen is another example.

I saw to-day a lady who has been under my care for a year or two with hallucinations somewhat similar to those referred to, sometimes associated with headache and sometimes independent of headache, but occurring with a peculiar nervous condition, and usually at night. It seems to me that it is important to relate these cases to functional disturbance of certain localized areas of the brain. It is well known that one of the theories of migraine is that it is in its essence an epilepsy, that it is an epileptiform or discharging disease. Suggestions of this kind help us to understand better the mechanism of such cases.

Dr. de Schweinitz: My object has been simply to add six cases to those reported by Dr. Mitchell. In the case of lateral hemianopsia in which visions preceded the development of the hemianopsia, Dr. Dercum made the post-mortem. The lesion was at the base, cutting off one optic tract, so that the higher visual centres were not affected. This, however, was an ill-defined case having no special connection with those reported to-night.

NON-INFECTIOUS FEVERS IN THE PUERPERAL STATE.

By BARTON COOKE HIRST, M.D.,
ASSOCIATE PROFESSOR OF OBSTETRICS, UNIVERSITY OF PENNSYLVANIA.

[Read April 3, 1889.]

The wonderful results of antisepsis in obstetric practice, the prominence of puerperal septicæmia as a subject of discussion in medical societies, journal articles, and more pretentious works, have accomplished a splendid purpose in directing men's minds to the constant danger, the ubiquitous nature, of septic poisons. But in one minor particular the result has been unfortunate. It is often difficult to convince a medical man that fever in childbed can have another origin than sepsis. I have had a personal experience in this direction. On one occasion I called in consultation a medical practitioner to a very critical case of fever in the puerperal state that, after careful study, I had decided was not of septic nature. My medical friend, after a cursory examination, assured me the case was one of puerperal septicæmia, and that the patient would die. The subsequent course and termination of the case demonstrated his mistake.

Fever during the puerperium, not due to infection, may arise from emotional causes, from exposure to cold, from constipation, from reflex irritation of any kind, from cerebral disease, from eclampsia, from insolation, from syphilis, from the exacerbation or persistence of an acute or chronic disease contracted before or during pregnancy. The limitation of the heading under which these affections are grouped excludes all consid-

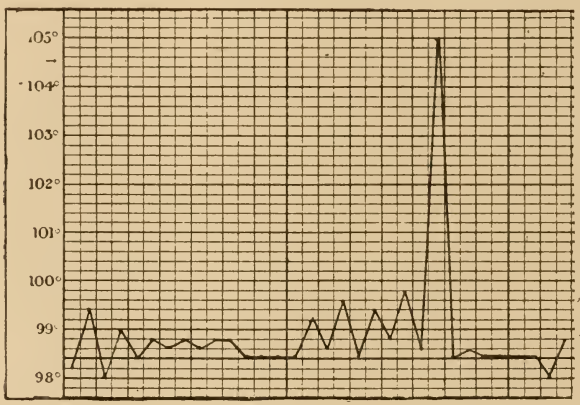
eration of septic infection and of the infectious fevers, as the exanthemata, erysipelas, typhoid fever, malaria, pneumonia.

That fever may appear in consequence of emotions, clinical evidence leaves no doubt. The explanation of its occurrence is of minor importance in comparison with a knowledge of the fact, for a comprehension of the former will not be required to treat such cases properly, if, indeed, they demand treatment at all, for, the cause of the fever being transient, perhaps momentary, the elevated temperature quickly sinks to the normal. It is not in every person that powerful emotion will be followed by an elevation of temperature to a noteworthy degree. There must apparently be predisposing causes in the nervous system of the individual. Emotional fever is not seen in men with well-poised minds used to the vicissitudes of life, opposing a resisting front to external impressions. It is most often met with in children, in hysterical girls, and in women after childbirth. In all these states, childhood, hysteria, and childbed, there is a curious irritability of the organism, a lack of control over mental processes. The petulant child, easily swayed by and completely yielding to emotions, subject on slight provocation to convulsions, is a familiar picture; and no one can overlook this same mental and nervous character in pregnancy and the early part of the puerperal state. It is this condition of the nervous system, apparently, that predisposes to emotional fever. A few years ago I had under my care in the infant asylum of the Philadelphia Hospital, a young child who was frequently thrown from slight cause into violent fits of anger. Immediately after these paroxysms the temperature would be raised, sometimes to 103°. Within an hour the fever disappeared.

Lawson Tait some time ago read before the London Obstetrical Society, a paper entitled "Some Observations on Puerperal Temperatures," based on records of 125 cases in which the temperature was taken every four hours; almost all of them had fever, and in quite a large number this is attributed to nervous origin. The number is doubtless over-estimated, for emotional fever in the puerperal state is not so frequently

seen as these statistics would lead one to suppose. Hunt's records of 75 cases, confined to women free from infection and inflammation, in which the temperature was taken twice a day in the mouth, give three apparently typical examples of fever from emotion. In one the cause is put down as "acute domestic infelicity"; in another, as ill-usage; and in the third, as anxiety. In the Maternity Hospital, designed for the reception of young girls illegitimately pregnant for the first time, I have seen several examples of emotional fevers. (Fig. 1.)

Fig. 1.



Emotional fever.

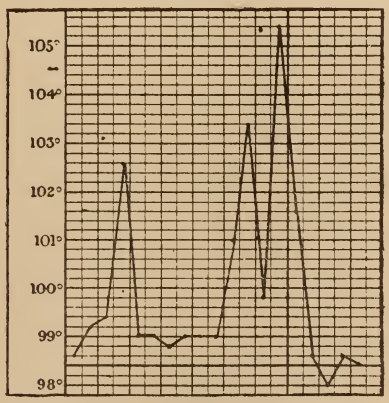
Failure to receive an expected letter, fear of exposure, the expected removal of the infant to an asylum, and dread of an operation for mammary abscess, have all been discovered as causes of the marked mental disturbance that produced fever. Perhaps the last case mentioned was the most typical.

There had been in the ward one operation for mammary abscess; it was witnessed by two puerperal patients. One of these, a young girl, shortly after experienced pain in her breast; she at once conceived a morbid dread of an operation. The beginning elevation of temperature in the chart indicates the commencement of engorgement and pain in the gland. This went on for a few days, when, after lying awake all night brooding on the subject,

the girl's temperature began to rise in the morning, finally reaching the height indicated on the chart. The only antipyretic employed was the emphatic assurance of the resident physician that there was not, and would not be, the slightest excuse for using a knife. The patient's fears being calmed, her temperature quickly sank to normal, where it remained.

Fever from Exposure to Cold.—In the sensitive condition of puerperæ it is not uncommon to see a febrile reaction follow undue exposure. A careless nurse or attendant may be responsible for too low a temperature in the lying-in room, or for ill-regulated ventilation, or for insufficient or ill-arranged bed-clothing. A wilful patient may leave her bed too soon and

Fig. 2.



Fever from exposure to cold.

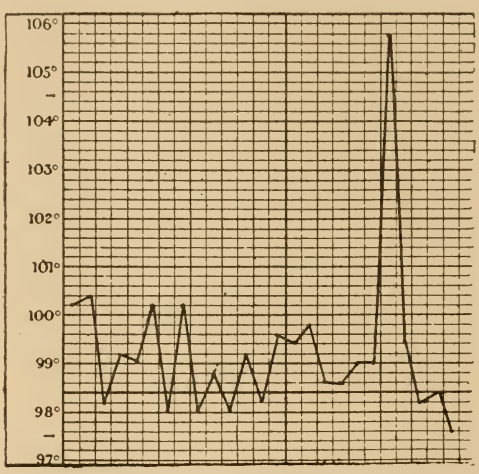
expose herself, thinly clad, to cold. I have seen a good illustrative case in the Maternity Hospital; it occurred in mid-winter.

Kate B., a private patient, left her bed on the twelfth day after labor, without permission, and very imperfectly dressed; some disturbance in the street attracted her to the window, where she stood quite a long time looking out. On the following day she had fever; she was kept in bed for four days, and, on arising, was again imprudent about her clothing. The fever returned with sharp pain in the abdomen, which was relieved by turpentine stupes. There was no infiltration to either side of the uterus, no evidence of infection. Finally, with greater care for body warmth, the woman left her bed and was troubled no more.

Constipation.—Schroeder says that "among the causes, aside from infection and local inflammations, which, with special frequency, produce fever in the puerperal state, over-distention of the intestines with fecal masses should be given a foremost place." This statement of an eminent authority assigns too important a *rôle*, I think, to constipation among the causes of elevated temperature after delivery. Every practitioner of obstetrics, however, has doubtless seen an example of this sort of "puerperal fever."

The temperature chart (Fig. 3) is from a woman in the Philadelphia Hospital who had but one evacuation of the bowels—on the fifth day—in the

Fig. 3.



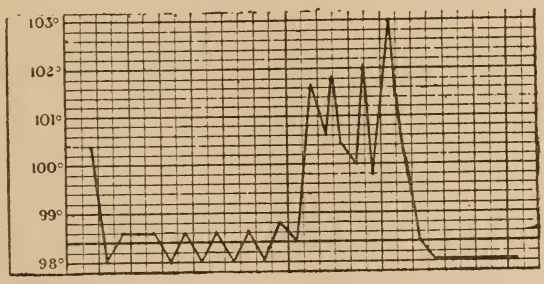
Fever from constipation.

eleven days succeeding delivery. The temperature rose to a great height, but fell almost immediately after the administration of an enema containing an ounce each of glycerine and hot water.

Fever from Reflex Irritation.—Physical irritation, as well as psychical, is often reflected in general elevation of the body-temperature during the puerperal state. The irritating-point is most often to be found in the breast. It is quite true that "milk-fever" as a constant occurrence in the lying-in woman

should no longer be looked for since the advent of antisepsis. But, nevertheless, there may be found in women of sensitive

Fig. 4.



Fever from congested breast.

nervous organism a well-marked fever which can be traced to no other cause than the engorgement and distention of the mammary glands. The appended temperature chart (Fig. 4) well illustrates this point.

A young primipara developed on the eighth day after delivery apparently an acute mastitis. The pain, the redness of the skin, the swelling of the breast, and the course of the temperature, all led me to believe that suppuration had occurred. Consequently, I cut deeply into the gland; there was free bleeding, but not a drop of pus was found. Immediately after the incision, which relieved the engorgement of the breast and the tension of the skin, the temperature began to fall with unbroken descent to normal.

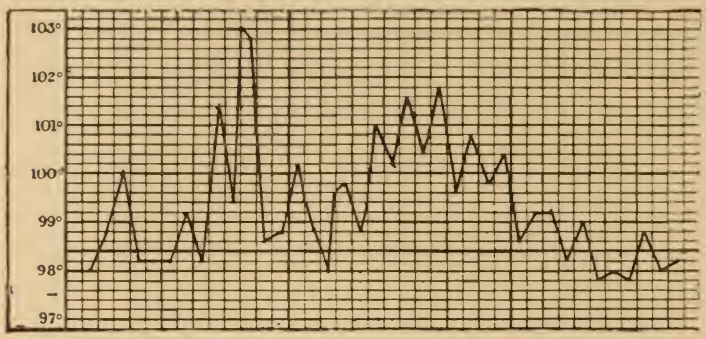
The focus of irritation is not always the mammary gland. A year ago I saw a most curious complication of the puerperal state.

A rather elderly primipara was delivered without difficulty of a healthy infant. In a short time the woman began to complain of a constant and distressing headache; diarrhœa came on, which resisted treatment, and the woman's mental state tended rapidly toward pronounced melancholia. With it all, the temperature rose in a suspicious curve. On the ninth day the body of a tapeworm, fourteen and a half feet long, was passed from the bowel, and shortly afterward the temperature dropped to normal.

It has seemed to me that the great elevation of temperature which often follows perforation into the peritoneal cavity,

coming on as it commonly does at once, should also be attributed more to an intense reflex irritation than to septicæmia.

Fig. 5.



Fever associated with tænia.

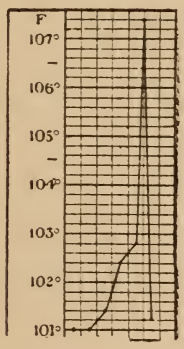
The appended chart (Fig. 6) is from a case in which the placenta was abnormally adherent. It was managed by one of my colleagues at the Philadelphia Hospital. Separation was accomplished four hours after delivery by means of the fingers and a curette. Ulceration of a limited area in the placental site followed, which ended in perforation and death on the third day.

Fever in the Puerperal State from Cerebral Disease.—A woman just delivered might have a tumor in the brain or spinal cord, insular sclerosis, locomotor ataxia, or degenerative changes in the brain—all of which could give rise to elevations of temperature. It is, however, to cerebral hemorrhages and embolism that one should usually look for an explanation of fever arising from brain disease, for these accidents are by no means rare in the child-bearing woman; and if the hemorrhage or embolism affect certain regions, a rise of temperature, often to a great height, is pretty sure to follow. A temperature of 108° in the axilla has been noted in a case of cerebral embolism following childbirth.

Fever with Eclampsia.—It is difficult to know where to place the fever that accompanies eclampsia. As very little is

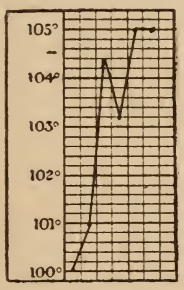
known about the etiology of the disease itself, so it cannot be expected that the origin of the fever will be explained (Fig. 7).

Fig. 6.



Fever following perforation of the uterus.

Fig. 7.



Fever of eclampsia. (From a fatal case in the Philadelphia Hospital.)

With our present knowledge it is safe to put it among the non-infectious fevers of the puerperal state. Winckel, writing in 1878, said he had observed and had called attention to the fever accompanying eclampsia fifteen years before; he was accordingly the first to refer to it. Bourneville and Budin published this fact as an original discovery in 1872.

With every convulsion there is usually a notable rise of temperature, until, finally, the fever may run very high.

Insolation.—Sunstroke or heat-stroke is by no means an impossible accident to lying-in women in the torrid temperature of summer in this part of the country. The only case, however, that I know of occurred at sea in a ship sailing from France to New Orleans. The cabin in which the woman was confined was hot and ill-ventilated. The temperature of the air was 93.4° Fahr. A portion of the membranes was left behind, and the discharge was offensive, but there was no fever. On the fourth day, however, the temperature rose to 104°, and shortly after mounted to 109.4° in the rectum. The woman ultimately recovered, in spite of Liebermeister's declaration

that death inevitably follows in man when the temperature rises to 108.5°.

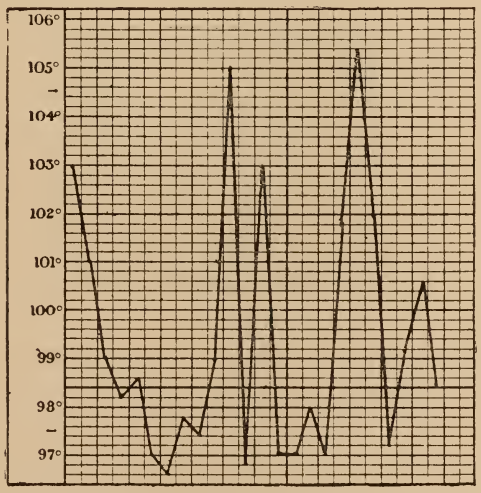
Syphilitic Fever.—Mewis, from an analysis of 167 cases of syphilis in lying-in women, made some years ago, came to the conclusion that the influence of the puerperal state upon the local lesions of the disease was a favorable one; but he called attention to a special tendency in syphilitic women to specific febrile action and to peri-uterine inflammations during that period. Fournier's discovery of a specific syphilitic fever naturally turned the attention of French writers and students to this matter, and there were four elaborate theses on the subject written in the years 1885–86 in Paris. According to Combes, the idea that syphilitic fever might complicate the puerperal state was first suggested by Pinard to Sacreste, who embodied this notion in a thesis. In his article the assertion is made that of 24 puerperal cases in which syphilis existed, 18 presented the specific fever. It is described as moderate, innocuous, lasting seven to eight days. These observations were made in such an astonishingly careless manner, however, that they are quite worthless. Mercier describes 9 cases among syphilitic puerperæ in which there developed fever, irregular, intermittent, or remittent in type, yielding to internal administration of mercury, and not influenced by antiseptic injections. Fauconnier studied the puerperal state of 36 syphilitic women: in 21 there was no fever; in 7 cases there was an elevation of temperature that could be explained by some complication of the puerperal state; in 8 instances nothing, it is asserted, could be found to account for the rise of temperature but the syphilitic condition.

Combes made a very careful and accurate investigation of the matter. His material for observation consisted of 188 cases in the services of Drs. Porak and Doléris. Of this number the lying-in was afebrile in 141. There was fever in 47—a proportion of 25 per cent. Of the latter number, 38 could be explained by the commoner complications of the puerperal state; 9 admitted of no definite explanation except the essential fever of syphilis; but in 2 of these cases infection had occurred more

than three years before, and they are therefore excluded. This leaves 7 cases of elevated temperature out of 188 confinements in syphilitic women to be attributed to syphilitic fever, and even this small number should perhaps be diminished by the subtraction of 3 others in which the duration of the disease was unknown, and could have been more than three years. Thus the proportion of syphilitic fever to be looked for in women after childbirth is only a trifle over 2 per cent. of those women affected with the disease. This estimate is probably correct.

I have never seen a case myself of syphilitic fever in the puerperal state, but once witnessed what I suppose is a typical example in a child with hereditary syphilis. (Fig. 8.) In my

Fig. 8.

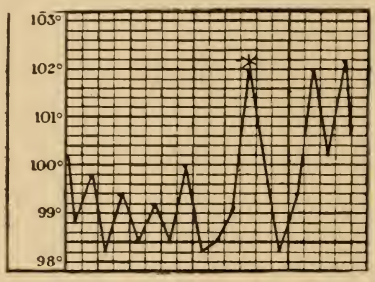


Syphilitic fever.

own experience with syphilitic women in childbed the disease has only interfered with puerperal convalescence in two ways: either by the retention within the uterus of the hypertrophied deciduous membrane, which is so often seen as a result of syphilis, or, as in one instance, by septic infection which occurred in consequence of large ulcerated surfaces in the vagina that had developed during pregnancy.

Persistence or Exacerbation of Febrile Affections in the Puerperal State.—A woman may, of course, acquire any of the acute or chronic fevers during pregnancy, which may persist in the lying-in period or take on new activity during that time. This is true of all the infectious diseases, but they will not be considered in detail here, with one exception. The effect of labor upon the course of phthisis has interested many observers. It has been asserted that the disease makes no progress, or at least is very much retarded, after delivery. This is certainly not my experience, nor is the belief a reasonable one. Processes that often exhaust a healthy woman could not exert a favorable influence upon a wasting disease. Indeed, we have the authority of Austin Flint for the belief that

Fig. 9.



Effect of labor on phthisis.

the anæmia of pregnancy, the drain of the puerperal discharges, and the excretion of so large a quantity of nutritious material during the period of lactation, are often accountable for the origin of phthisis in women with tuberculous tendencies. In my own cases there has usually been, after delivery, an exacerbation of fever, with an aggravation of the pulmonary symptoms (see Fig. 9). The asterisk indicates the completion of labor.

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A CASE OF ACUTE ABSCESS OF THE TONGUE.

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[Read April 3, 1889.]

J. M., aged thirty-four years, consulted me some time ago in reference to the condition of his tongue, which he said for a few days had caused him a little pain and felt a little rigid upon motion; he himself was inclined to think he had irritated the organ by more than his usual amount of smoking.

Upon examination of the organ I observed that it was not markedly enlarged, was covered by a whitish-yellow fur, and seemed a little indurated, particularly on the right side near the median line. I was disposed to accept the patient's explanation of the cause of his discomfort, and advised him to abstain from smoking for a few days, and ordered for him a mouth-wash of chlorate of potassium, tr. myrrh, glycerine and water, and asked him to report to me if he experienced further trouble.

Three days later I was asked to see him at his home in the country, and when I called upon him I found him suffering great discomfort from the painful and swollen condition of his tongue, which apparently nearly filled the cavity of the mouth and projected some distance beyond the line of the teeth; the organ was greatly thickened and its lower surface was deeply indented by the lower incisors. The dorsal surface of the organ was covered by a thick, yellowish fur, and there was a profuse dribbling of saliva from the mouth. He was able to make himself understood with great difficulty; but I learned from him that during the previous twenty-four hours his tongue had become painful and had increased rapidly in size, and that he was able to swallow only liquids, and these with great difficulty. He also stated that he had not been able to rest in the recumbent posture on account of the profuse salivation which caused frequent efforts at deglutition which were painful, and he also experienced increased difficulty when he assumed this position. For these reasons he had spent the greater part of the night sitting in a chair by his bed with his head bent forward, thus allowing the saliva to escape freely from his mouth, as this was the position in which he experienced the least discomfort.

Examination of the dorsal surface of the tongue with the finger showed

that the organ was much indurated, and just to the right of the median line there was an area of tissue under which I thought I could detect deep fluctuation. I therefore made an incision about an inch, or an inch and a half, in length through the tissues with a bistoury at this spot into the substance of the organ, and upon introducing a grooved director through the incision there escaped several drachms of thick purulent matter; the amount of bleeding was insignificant.

Within half an hour the patient expressed himself as much relieved, both as regarded the pain and the difficulty in deglutition and breathing.

There was some purulent discharge from the wound in the tongue for the next twenty-four hours, and the swelling diminished rapidly, and in three days the organ had resumed its normal size, and there remained only a little induration about the seat of the incision.

Acute abscess of the tongue is a rather rare affection, and it is said to be more commonly met with in young adults than in children or aged persons. Dr. Gross[1] speaks of the rarity of acute abscess of the tongue, and states that he has never seen a case of acute glossitis terminate in abscess.

Mr. Erichsen[2] speaks of acute abscess of the tongue as a rare affection, but states that he has seen several cases. Both Professor Agnew and Professor Ashhurst speak of the rarity of the occurrence of this affection.

Mr. Holmes, Mr. Butlin, and Mr. Fairlie Clark record a few cases of this affection.

When acute abscess of the tongue occurs it usually results from acute glossitis, and this affection has been variously attributed by different observers to exposure to damp and cold, to injury, to the entrance of septic matters, bites of animals, stings of insects, to the injudicious use of mercury, to corrosive substances, and the influence of certain fevers and eruptive diseases.

Butlin,[3] DeMussy, and Duckworth are inclined to look upon acute glossitis as a catarrhal affection; the former authority says that it may result from the direct action of exposure of the organ to cold, but that in the majority of cases the exposure

[1] System of Surgery, vol. ii. p. 445.

[2] Science and Art of Surgery, vol. ii. p. 464.

[3] Diseases of the Tongue, p. 40.

has been indirect and general, and the inflammation must be regarded as reflex, possibly due, as has been suggested, to reflex irritation of the lingual division of the fifth nerve.

Mr. Holmes thinks that acute glossitis is more apt to be the result of general influences than of local irritation. Excessive smoking, and the too free use of alcohol, have also been considered to bear a causal relation to the development of acute glossitis, and I have myself seen this affection occur in a patient during an attack of delirium tremens.

Fairlie Clark[1] says that acute abscess of the tongue is usually preceded by more or less glossitis, and mentions three cases of this affection, in one of which a fatal result followed; in this case the beard of a grain of barley had punctured the under surface of a tongue, and had given rise to an acute abscess at the root of the organ; the patient died on the seventh day, and an abscess the size of a turkey's egg was found, upon post-mortem examination, occupying the position of the genio-hyoglossus muscle and the substance of the tongue; the foreign body was found in the abscess cavity.

Dr. Evans,[2] in a series of sixteen cases of acute glossitis which he collected, found that three of the cases terminated in abscess.

Dr. W. Meyers[3] reported several cases of acute glossitis, one of which terminated in acute abscess.

In acute abscess of the tongue the onset of inflammatory symptoms is very rapid, and the symptoms presented are practically those of acute glossitis. The patient complains of tenderness of the organ in chewing solid food, and of a severe stiffness or rigidity in the tongue, which is accompanied by more or less pain, and there is sometimes experienced pain in the muscles of the submaxillary region. In a few hours the tongue begins to swell, and this swelling rapidly increases in size, which is accounted for by the pressure upon the veins and lymphatics, and the organ is protruded from the mouth, and is

[1] Ibid., p. 130. [2] London Lancet, 1886, vol. ii. p. 724.
[3] Cincinnati Lancet and Clinic, vol. ii. p. 7.

indented by the teeth. The dorsal surface of the organ is covered by a heavy yellowish or brownish fur, and the surface may be dry if it has been long protruded from the mouth; there is also profuse salivation, and the salivary and lymphatic glands may be markedly swollen.

A patient suffering from acute abscess of the tongue experiences difficulty in talking, and dysphagia may be a more or less marked symptom, and if the organ is greatly enlarged, so as to fill the cavity of the mouth and extend back into the pharynx, the resulting dyspnœa may be so marked as to threaten the life of the patient.

Acute abscess of the tongue may open spontaneously, or there may be localized sloughing of the organ. Mr. Fowler[1] reports a case of acute glossitis which terminated in an abscess in which a spontaneous opening occurred.

The number of fatal results following acute suppuration of the tongue has been sufficiently large to show that the affection is a serious one; the deaths in these cases resulted from dyspnœa, septicæmia, or from pneumonia.

The treatment of acute abscess of the tongue is very similar to that of acute glossitis, and consists in making free incisions in the organ to permit of the escape of the purulent matter. I think that the now generally adopted treatment of acute glossitis by free incisions into the inflamed organ, prevents, in the majority of cases, the development of acute abscess of the tongue.

If dyspnœa be a prominent symptom, and it is not relieved after the tongue has been freely incised, tracheotomy may then be required, or, if the case be very urgent, laryngotomy, from the ease and rapidity of its performance, may be substituted for the former procedure.

[1] London Lancet, January 22, 1881.

REPORT ON THE TREATMENT OF FOURTEEN CASES OF DISEASE OF THE SPINAL CORD, BY THE METHOD OF SUSPENSION.

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[Read May 1, 1889.]

ALTHOUGH the treatment of locomotor ataxia by suspension was introduced in 1883, by Motchoukowsky, of Odessa, in Russia, the method received no extended trial in that country, and failed to attract notice elsewhere until the attention of Prof. Charcot was directed to it last autumn. Since the date[1] of the publication of the latter's first lecture on the subject, the method has received such publicity through the journals, that a description here is unnecessary.[2]

In cord affections occurring secondarily to disease of the vertebræ this method was advocated, and practised with good results, so long ago as 1826, by Prof. J. K. Mitchell, of the Jefferson Medical College, and has been employed for a number of years by S. Weir Mitchell[3] in the same class of cases. The latter, indeed, about the time of Motchoukowsky's discovery, was induced to attempt suspension in two cases of

[1] Progrès Médical, January 19, 1889.

[2] For details, vide Progrès Médical, January 19 and February 23, 1889; Rev. Gén. de Clinique, February 14, 1889; and The Treatment of Locomotor Ataxia by Suspension; translated from the French of Prof. Charcot, and edited by H. de Watteville, M.D. D. Stott, London.

[3] Vide Treatment of Pott's Paralysis by Suspension, etc., by S. Weir Mitchell, M.D. American Journal of the Medical Sciences, May, 1889.

spastic paraplegia in which no spinal caries existed; the excellent results obtained from it in pressure paralysis having led him to believe that stretching the cord, and thus altering its vascular conditions, even when signs of meningeal inflammation were absent, would helpfully affect both cord and membranes. Unfortunately, in these cases, no benefit followed; had it been otherwise, suspension would, no doubt, long since have been generally recognized as a useful adjunct to our scanty therapy in chronic degenerations of the spinal cord.

Closely following the introduction of the method into France it has been used in many forms of disease of the cord, in various forms of chronic degeneration of the nervous system, and in functional nervous trouble, such as neurasthenia; but, thus far, no report has appeared of a sufficiently extended trial in these to justify positive conclusions as to its utility. I have not, as yet, suspended more than a few times, at very irregular intervals, cases other than those of cord disease. Efforts are now being made to induce a considerable number of chronic nervous cases, which have been intermittently under observation in the out-patient medical department of the Jefferson Hospital for some time, and which, seemingly, are such as might receive benefit from this treatment, to submit to its trial. Later, a report will be made on suspension in these. The fourteen on which this paper is based comprise eight tabetics, three cases of spastic paralysis, two of ataxic paraplegia, and one of myelitis. The following are brief histories of these:

Case I. *Locomotor ataxia; second stage.*—Clement N., aged thirty-eight years; waiter. First seen January 23, 1889. He then had had severe lightning pains for a year or more. There had been present from about the same time incoördination of movement, especially in the dark. This was increasing. There was heightened sexual desire, with nocturnal losses, constipation, and vesical incontinence. Swaying was so decided, with feet together and eyes closed, that he would fall if unsupported. He had lost twenty pounds in weight in eight months. The upper extremities were not involved. There were no eye symptoms. There were plantar anæsthesia, with delay in transmission of an impression, perverted temperature sense, and absent knee-jerk. Had syphilis in 1879.

He was placed on mercuric iodide until treatment by suspension was begun,

on February 18th. The only change in his condition then was slight diminution in the severity of the pains. He has been suspended, in all, thirty-six times, usually four times a week, Monday, Wednesday, Thursday, and Saturday being the days chosen. The duration of the first suspension was one minute; the second, one and a half minutes; the third, two and a half minutes; the fourth, three minutes; all the subsequent suspensions were from four to five minutes, usually the latter. He was swung entirely from the ground in all the suspensions. The first three suspensions were by the head alone, but so much spinal pain was felt during them that, thereafter, shoulder-straps were also used, he being directed to raise and lower the arms slowly three or four times in a minute. At the fifth suspension marked improvement was apparent; his gait was decidedly better; static ataxia had lessened materially, the lightning pains had decreased in severity and frequency, and plantar anæsthesia had diminished.

During the second week of treatment the pains entirely disappeared, and have not returned. Since then improvement has been gradual, though continuous, to the present. The increased sexual desire, constipation, and vesical incontinence have disappeared. He is now able to walk with eyes closed; static ataxia is slight; subjective sensation in the plantar surfaces is good, and the two points of the æsthesiometer are now felt at the normal distance; temperature sense is normal; the knee-jerk has not returned.

Case II. *Locomotor ataxia; second stage.*—William E. T., aged forty-six; clerk. Had been under observation but a short time when suspension was begun. The following is an abstract of the symptoms noted the day of the first suspension: He had *petit mal* for one and a half years preceding the onset of tabetic symptoms three years ago. There then appeared lightning pains, which continued intermittingly until suspension was begun. Before the first suspension they were very severe, and were aggravated in damp weather. Sexual desire was first increased and then lost. There were constipation, alternating with fecal incontinence, and loss of vesical control. The gait is ataxic; there is staggering in the dark and in turning. Swaying is decided with feet together and eyes closed. The upper extremities are unaffected. There is decided tactile anæsthesia of the lower limbs. Temperature sense is perverted. Tactile and temperature senses are decidedly delayed. Knee-jerk is absent. There are no eye symptoms. He is melancholic; attributes his disease to mental worry. No specific history.

He was first suspended February 15th, and subsequently three to four times weekly to the present; but occasionally there was an interval of several days between. The first four suspensions were entirely by the head; in the later ones shoulder-straps were used, the arms being raised and lowered as in the other cases. First suspension occupied three-quarters of a minute; the second, two minutes; the third and fourth, two and a half minutes; and all the subsequent ones from two to five minutes, the average being three minutes. He was rarely suspended five minutes, because of severe pain always

appearing in the cervical and dorsal region when the treatment was prolonged. He thinks the improvement better when the time of suspension is shorter. After the fourth suspension there was improvement in the gait and decided diminution in the severity of the lightning pains, though the weather was bad for some time. After the seventeenth suspension, vesical and fecal incontinence entirely ceased, and has not returned. The improvement in his gait had then begun to be remarked upon by all his acquaintances.

He has now been suspended thirty-two times. There has been no change in his condition since the twenty-fifth suspension. It was then noted that his walk and station were far less ataxic than at first. There is a most decided improvement in his mental condition; cheerfulness has replaced despondency; he has gained strength; appetite and sleep are better than at first. The lightning pains no longer occur; rheumatoid pains are occasionally felt prior to a storm, but are of no severity. Sexual desire and capacity have not returned. Knee-jerk remains absent. There is less tactile anæsthesia in the lower limbs; sensation, however, is still delayed. He can distinguish heat and cold, but there is delay in transmission as before.

Case III. *Locomotor ataxia; second stage.*—John Van S., aged fifty-three. Has been under observation for ten years. Knee-jerk has not been present during that time. Gait has been ataxic for twenty years; anæsthesia and lancinating pains have been present for the same period. There was loss of sexual power and desire, which returned somewhat for a time, with, also, some amelioration in the other symptoms, under the salts of silver and potassium iodide; but, prior to the first suspension, locomotion without two canes had been impossible for seven years. The gait was typically ataxic, and there was retroflexion of the knee-joints. Lightning pains were severe and constant. Tactile sense was diminished; temperature sense was normal. There was concentric limitation of the visual field with a normal fundus.

The first suspension (one-half minute) was on February 20th; the second (two minutes) on the 25th; the third (two minutes) on the 27th. Since then he has been suspended from three to four times weekly, receiving in all thirty suspensions, which, after the fourth, were of five minutes' duration. No pains occurred during them. The method was the same as in the preceding cases. The lancinating pains disappeared the day following the third suspension, and have not returned. On the day of the fourth suspension (twelfth treatment) he appeared at the hospital without a cane, coming a distance of several miles, and walking a fair part of the way; the first time he had done without support in locomotion in seven years. Subjective sensation in the plantar surfaces was also better, and he expressed himself as feeling "like an old buck." This improvement in gait has been continuous to the present, and is remarked in wonder by his family and neighbors. Sexual power and tactile sense have improved. The knee-jerk has not returned.

CASE IV.[1] *Locomotor ataxia; second stage.*—George L., aged sixty-three; schoolmaster. Seven years ago lightning pains, anæsthesia of the lower limbs, and an ataxic gait appeared, with loss of sexual desire. An examination immediately before suspension was begun showed markedly ataxic walk and station; absent knee-jerk; decided tactile anæsthesia in the extremities; Argyll-Robertson pupil. There were girdle and shooting pains. No specific history. He was at first suspended every day for three weeks; and subsequently on alternate days. The average length of each suspension was two and three-quarters minutes. There has been a very distinct improvement in gait, and incoördination is slight compared with what it was. The girdle sensation has disappeared, and the lancinating pains, though not entirely gone, have much ameliorated. The knee-jerk has not returned, and the reflex immobility of the pupils and loss of sexual desire continue. The mental condition has improved.

CASE V. *Locomotor ataxia; first stage.*—James L. E., aged forty; brakeman. For several years he had characteristic lightning pains, and recently has noticed uncertainty of walk in the dark. With closed eyes there is bad station. The gait is not ataxic. The knee-jerk is absent; no reinforcement. There is Argyll-Robertson pupil. He has been suspended four times. After the second suspension there was a decided lessening in the severity of the pains. This case came to the hospital from a distance, unprepared to remain longer than a week. A report on his condition will be made at another time, after further suspensions.

CASE VI. *Locomotor ataxia; second stage.*—Mrs. M. C., aged forty-eight; general housework. Since September, 1883, she has had paralysis of the left third nerve, causing mydriasis, external strabismus, and almost complete ptosis. In 1887, locomotion became difficult, at first only in the dark, but soon in the light. She has had lightning pains for over a year, and rheumatoid pains occur in the joints, worse in damp weather. There is a constant sensation of burning and weight in the dorso-lumbar region, and the spinous processes of the vertebræ in this situation are decidedly tender on firm pressure. There are: staggering gait; inability to stand with feet together and eyes open; absent knee-jerk; pronounced tactile anæsthesia in the lower limbs, and especially in the plantar surfaces; normal temperature sense. An examination of the eye showed hypermetropia; a normal fundus; total paralysis of the left third nerve; pupils irregular in outline, responding sluggishly to light, and immobile to accommodation. The above points were noted when she first came under observation, two months before beginning suspension, and again immediately before. She had received no benefit from medical treatment.

The first suspension was on February 15th, and the last on March 30th, she receiving it in all twenty-one times, usually four times a week. The first

[1] Patient of Dr. E. Hershey's.

two suspensions occupied three-quarters of a minute ; the third, one minute ; the subsequent ones from two to five minutes, usually the latter. The method was similar to that pursued in Case I., and the intervals were the same. The first three suspensions were accompanied and succeeded by an increase in the sensations of burning and weight in the dorso-lumbar region; this, later, disappeared, but returned occasionally while suspension was continued. The tenderness over the spinous processes, however, entirely disappeared. The shooting pains were decidedly ameliorated, and station improved after the sixth suspension. The ptosis was noted to be much less at the date of the twelfth suspension. Other than these there were no changes. Loss of knee-jerk, tactile sensation, and sexual desire persisted. After a six weeks' trial she ceased to return for suspension.

Case VII. *Locomotor ataxia ; second stage.*—Mrs. R. C., aged forty-nine; housewife. The following points were noted shortly prior to beginning suspension. Has had characteristic lightning-like pains for fifteen years, and for the past six years an ataxic gait ; sensations of numbness from the waist downward; a girdle pain, and absent sexual desire. There were present very bad station; absent knee-jerk ; decided tactile anæsthesia in the lower extremities, but normal temperature sense. There were Argyll-Robertson pupil and beginning atrophy of the left optic nerve. Because of being light in weight, she was suspended by the head alone.

The duration of each suspension varied between three-quarters of a minute to four minutes. She was suspended in all ten times. At the visit for the fifth suspension it was noted that her gait and station had decidedly improved ; she felt stronger physically, was in better spirits, and had little or no pain. After the sixth suspension the ataxia had lessened so considerably that she presented herself for the seventh suspension (the thirteenth day) walking entirely without support—for the first time in several years. The improvement in gait and station and the lessening in the pains were maintained; but at the date of the ninth suspension there was no return of knee-jerk, and tactile and sexual anæsthesia remained. The eye symptoms were unchanged. She complained of having trouble at home, and of being much overworked, and did not return for treatment after the tenth suspension—why, I am unaware, as she stated at the last visit that she was much gratified with the improvement.

Case VIII. *Locomotor ataxia; second stage.*—Mrs. K. M., aged forty-five years; housework. The following symptoms were noted just prior to beginning suspension. Has had generalized neuralgiform pains for twelve years, with occasional lightning-like pains down the thighs. Has been somewhat ataxic for a number of years. There are decided static ataxia, absent knee-jerk, paræsthesiæ in the lower limbs, diminished and delayed tactile and temperature sense in the plantar surfaces, absent sexual desire, Argyll-Robertson pupil. She was suspended in all eight times, and then was forced to discontinue her visits because of contracting a severe cold. At the time of

the last suspension the only improvement noticeable was in the ataxia. The walk was normal, and there was far better station.

CASE IX. *Primary spastic paraplegia.*—Henry S. A., aged thirty-seven years; cake-baker. Eleven years ago there gradually appeared weakness in the lower limbs, with rigidity and tremors. For several years has been totally unable to walk without crutches. Immediately prior to beginning suspension it was noted that he could not walk more than a few steps with them. The gait was typically spastic, and the limbs shook as he progressed, from the clonus produced as the ball of each foot rested alternately on the floor. The lower limbs were rigid: the knee-jerk was much heightened, ankle-clonus was very decided, and there was a general increase in the tendon and periosteal reactions. Sensation was normal; coördination, also, as far as it could be tested. There had been obstinate constipation for years, and, occasionally, rheumatoid arthritic pains.[1] No specific history.

Suspension was begun on February 25th, and continued four times weekly until March 25th, when he returned to his home in Camden. Though compelled to depart, he was so unwilling to abandon a treatment which seemed to be productive of so much good that he purchased an apparatus and has suspended himself daily from March 26th.[2] Allowing for psychical effect—for he has become a decided enthusiast on the method—the improvement in his condition has been marked. The day following the second suspension he stated the limbs felt less stiff, their movements were freer and more under control, and that locomotion with the aid of crutches was less difficult; this at the time of the fourth suspension was apparent to myself and others who then saw him. There was less spasm and rigidity, and the gait had improved. Coldness of the feet had been constantly present before suspension was begun, but disappeared after the first hanging, and has not returned. He recently wrote to me, in reply to an inquiry as to his condition, that he had discarded his crutches, and now goes about the house readily enough with the aid of two canes; that he stands without fear of falling, and that there is freer movement in the feet, and less tremor and spasm in the limbs than there has been for ten years.

CASE X. *Primary spastic paraplegia.*—George F., aged thirty-eight years; shop-keeper. Five years ago the lower limbs became gradually weak, easily fatigued, and were the seat of stiffness and occasional spasm. A typical spastic condition is present. The walk is spasmodic, the knee-jerk much exaggerated, and there is a decided ankle-clonus. There are no disturbances of sensation or coördination. The pupils are normal. There is

[1] It is interesting to note that, when he first presented himself for treatment, one and a half years ago, and was under observation one week in the wards, his occupation and the eliciting of the last mentioned symptoms, caused me to suspect lead poisoning. His urine was then examined by Prof. Leffmann, who reported that it contained considerable lead.

[2] He has now had forty-five suspensions.

beginning atrophy of the right optic nerve. He had syphilis seventeen years ago. He has been suspended thirty-four times; method, duration, and intervals the same as in Case I. There has been no decided improvement in his condition. After the seventh suspension he stated he had begun to feel in higher spirits, and ate and slept better. He thought the limbs were not so weak and rigid as formerly. I, however, noticed no difference then, but, on examining him the day of the tenth suspension, there seemed less spasticity, and his walk had improved, but the tendon reflexes continued exaggerated as before. Since then the improvement in gait has not been marked. There is now, however, much less fatigue felt in the legs than formerly, and less tendency to spasm. The knee-jerk continues heightened, but the foot-clonus is less easily elicited, and is feeble.

Case XI. *Primary spastic paraplegia.*—Samuel G., aged fifty-three years. A year ago there gradually developed weakness and heaviness in the legs, and a tendency to jerking of the muscles. Several attacks of colic preceded the appearance of these symptoms. There is weakness in the legs on slight exertion, and the gait is slightly spastic. The knee-jerk is increased, and a clonus can be produced in the left ankle. Tactile sensation and coördination are unimpaired. There are no electrical changes, and no specific history. There is atrophy of the left optic nerve. He was suspended thirty-four times; the method, duration, intervals, etc., were the same as in Case I. There has been no improvement in his condition, save that he has lost a feeling of nervousness and dread that was very prominent before suspension was begun. This disappeared in the second week.

Case XII. *Combined lateral and posterior sclerosis.*—Henry S., aged forty years; shuttlemaker. He visited the clinic once, five years ago, in search of a diagnosis. The following points, with others, in his history, were then noticed: Staggering gait: left knee-jerk much increased; incoördination; cannot walk or stand without assistance; impaired grasp, both hands; shooting pains in the extremities; pruritus; constipation; incontinence of urine; normal tactile sense; double optic neuritis; loss of color sense; peripheral vision better than central; no specific history. He was recently invited to present himself for suspension. Additional points noted at the date of the first suspension are, that the first symptoms appeared ten years ago, and were those of combined lateral and posterior sclerosis. There is now present a spastic gait; inability to walk or stand unsupported; rigid limbs; much exaggerated knee-jerk; very decided ankle-clonus; pronounced incoördination in both upper and lower extremities; normal tactile and temperature sense; unimpaired sexual power; chest pains; constipation; no bladder difficulty; advanced double optic atrophy and limitation of the visual field; pupils normal in size and immobile. He received nine suspensions. At the time of the third suspension he felt decidedly stronger and better; limbs were more supple; had walked more in the two preceding days than for three or four years. The fourth and fifth suspensions caused severe pain in the

back and limbs, and slight faintness. After the ninth suspension he abandoned the treatment, believing that the distance he was compelled to travel to be suspended overcame any benefit he received.

CASE XIII. *Combined lateral and posterior sclerosis.*—John S., aged thirty-one years; miner. Has had lightning-like pains in the extremities, more or less constantly, for three and a half years, and dull aching in the lumbar and sacral regions. Motor weakness and incoördination in the legs, with stiffness and tremors, appeared shortly after the onset of the pains, and are increasing. He is unable to walk with closed eyes. The gait is spastic. The leg muscles have dwindled. There is slight plantar anæsthesia and delay in the transmission of a tactile impression. The knee-jerk is very active; ankle-clonus is pronounced. The arms are normal. There is no bladder or rectal trouble. Sexual power is impaired. No specific history. He believes the disease was induced by constant exposure to wet cold. He was suspended six times, and then returned home. Subsequently he used suspension on alternate days for six weeks without my knowledge, and he writes that he has now but slight pain, and that locomotion is "almost as good as ever."

CASE XIV. *Subacute dorso-lumbar meningo-myelitis.*—John B., aged twenty-nine; miner. In March, 1888, five weeks after a fall upon the buttocks, in which he felt himself much shaken, there appeared difficulty in locomotion; lancinating pains in the course of the ilio-hypogastric and ilio-inguinal nerves, and in the thighs, which continued constantly for several months. The pains were of great severity, and necessitated the frequent hypodermatic use of morphine for their relief. In August, the loss of power became absolute; he was then unable to walk, even with crutches. I saw him first on February 28, 1889, when he was brought to the hospital for treatment. There was then total paraplegia. He could not maintain the erect posture, though held by two men. The legs and thighs were somewhat wasted; a spastic condition was developing, and reaction of degeneration had appeared in the anterior thigh muscles. The spinous processes of the first and second lumbar vertebræ were decidedly tender on pressure, and the vertebral points below this were slightly so. There was marked diminution in tactile perception in the lower limbs; the two points of the æsthesiometer being felt as one at their greatest separation. Heat and cold applied to the plantar surfaces seemed equally hot, and the touch of a feather produced a burning sensation. A very faint plantar reflex was present, but no gluteal or cremasteric. Knee-jerk was just perceptible on the right; a trifle better on the left, but much enfeebled. A hot sponge drawn down the spine showed a hyperæsthetic zone in the lower dorsal region.

Suspension was begun on February 28th. The neck and shoulder straps were used. He was always drawn entirely from the floor, and the arms were raised and lowered at intervals, as in the other cases. The first suspension occupied one minute; the second three minutes; the fourth four and a

half minutes; all the subsequent ones five minutes. He has now received thirty-one suspensions, and the improvement in the paraplegia has been remarkable. After the second suspension he was able to stand with crutches, and, at the time of the fourth, he got about the wards with their help, and, since April 1st (eighteen suspensions), he has been able to walk a short distance with the aid of one cane. The rigid condition of the limbs has lessened, and their nutrition is better. The tenderness over the lumbar vertebræ had entirely disappeared by the latter part of March. Pains occasionally appear in the interval of the suspensions in the course of the ilio-hypogastric nerves, but are always dispersed by a suspension. The knee-jerks are somewhat more active than normal. The plantar reflex has improved; the gluteal and cremasteric remain absent. Anæsthesia in the plantar surfaces has lessened very materially.

The total number of suspensions made in these cases is upward of three hundred and fifty-two. An extended analysis of the results obtained is unnecessary. It may be seen by a glance at the histories that, in the tabetics, the symptoms which were first and most strikingly relieved were the ataxia and the lancinating pains; improvement in these occurred in all,[1] and after a very few suspensions; and that, if any relation existed between the early appearance and extent of the improvement and the duration of the disease, it is a direct one; the more chronic cases being those most promptly and decidedly benefited. The improvement in these two symptoms, the most prominent and distressing of the disease, in at least two of the cases (III. and VII.) was so prompt and decided that it may well be considered truly remarkable.

Bladder and rectal difficulty, present in two of the tabetics, and increased sexual desire present in one, have disappeared. Diminution in, or loss of sexual desire, present in five, has improved in but one. Tactile anæsthesia has much improved in three and slightly in one; not at all in four. A somewhat melancholic condition, present in five of the tabetics, disappeared after a few suspensions. This was not due directly to the suspensions, but was rather secondary to the lessening of the pains and ataxia. In none of the eight has the knee-jerk returned, even with reinforcement.

[1] No pains were present in Case VIII. while under treatment by suspension.

In the three spastic cases, diminution in the paralysis, rigidity, and spasm has been striking in one and slight in another; in the third, no change has occurred in these symptoms.

In one of the ataxic paraplegic cases, ataxia and paralysis seemed to be lessened after the second suspension. Unfortunately, this improvement was not maintained, possibly because suspension was not persevered in. In the other case, there has been a decided lessening in the pain, and the ataxia and paralysis have greatly diminished.

In the case of myelitis, the improvement in the paraplegia and in the general condition has been decided.

Eye symptoms were present in the fourteen; have undergone no change.

As these cases are few in number, and have been under treatment by suspension but a comparatively short time, no very definite deductions can yet be drawn as to the exact value of this method in the treatment of degenerative and inflammatory diseases of the cord. It may, however, be safely asserted, from the favorable results obtained in nearly all of the tabetic cases, that suspension is of great service in locomotor ataxia, but, whether palliative merely or actually curative, the time has not arrived for definite reply. It is certainly of use in lateral sclerosis; and in myelitis, the decided improvement produced in one case, in which absolute paraplegia had existed for over six months, commends its trial in others. That suspension will, even though aided by wisely directed medical and hygienic means, effect a cure in degenerative disease of the cord is, I think, doubtful; but, as an adjunct to other modes of treatment, its utility is beyond question.

The recent plausible explanation as to the action of suspension in tabes, offered by Althaus,[1] indicates that permanent benefit may result from this method, provided that the further progress of the disease can be arrested by other means, and that it has not advanced to complete destruction of the nerve

[1] Lancet, 1889.

tubules; and that, even where its advance cannot be thus stayed, temporary good may still be accomplished by suspension. Althaus's ingenious explanation applies equally to other degenerative diseases of the cord than tabes, and if it is true of the chronic degenerations, in which the pathological changes originate in the nerve elements and the overgrowth of connective tissue is secondary, it is, theoretically, far more so of inflammatory processes which, starting in the connective tissue, cause, primarily at least, far less damage to the nerve tubules. In chronic inflammations of the cord or its investing membranes which are stationary and are encountered before the contracting perineal tissue has, by compression, done more than interfere with the nutrition and conductivity of the fibres, not irreparably damaging their structure, the effect of suspension should be curative; for, by stretching the cord and the thickened and adherent membranes, the inflammatory tissue pressing upon the axis-cylinders will be loosened and broken down, setting free the latter and permitting them to resume their functions.

DISCUSSION.

Dr. S. Weir Mitchell: I want, in the first place, to thank Dr. Stewart for this interesting communication, because it has brought up a subject which is now coming to be considered by many of us from a very practical point of view. Many have considered this matter as merely an unusual method of treatment which had in its favor chiefly its extraordinary character. It is well, therefore, to have some one show that it has distinct value. I myself have been using this method in common with some of my colleagues at the Infirmary for Nervous Diseases ever since the first accounts of it were published. As Dr. Stewart justly remarked, it was not altogether an unfamiliar method to me. I must look upon it as one of those pieces of ill-luck which sometimes come to all doctors, that, when I employed a plan of treatment almost identical with this some three and a half or four years ago, I should have met at the outset with two cases in which it did no good. This prevented further trial. If I had been fortunate, I no doubt would have continued the treatment. I had been perfectly certain that in a good many cases of spinal disease which my father had relieved of paralysis, or which in

my hands were aided by his method of stretching from the head, there had been no gain in the curvature, and that the favorable changes must have resulted from some of the processes which are active in the present method of treatment. There is a certain identity between the means employed and the results obtained. May I ask Dr. Stewart if he used the apparatus of Charcot or the modified apparatus?

Dr. Stewart: I used the ordinary apparatus.

Dr. Mitchell: I ask this for a reason. In the apparatus used by the Russian, and afterward by Charcot, the suspension was made from the arms, and at the same time a stretch was made of the neck. We found some difficulty with the apparatus of Charcot. Patients were often numb in the hands, and staggered after a suspension of only a few minutes. They also complained of great pain in the back, so that it was impossible to suspend them for longer than a minute. One day, picking up a child by its elbows, it flashed across my mind that that was the way to accomplish suspension. I then had the apparatus made which you see before you. In this the elbows are placed in slings, and a band around the waist keeps them from getting away from the chest. In such an apparatus a patient can be suspended for twenty minutes without discomfort. The pull on the head is accomplished by a separate pulley. By this you can regulate the force applied to the head, and can at times make an extra pull, such as Charcot enjoins as part of the treatment. He secures this by having the patient throw out his arms at intervals.

It occurred to me that in the European accounts there was no reference to the physiological effects of this method of treatment. I have made some observations in this direction, and have here a tentative report of the results. My resident, Dr. Otto, has taken great pains in preparing this report. I wanted, first, to know how much a man was stretched in this apparatus. Dr. Otto suggested that it would be proper to measure the subject from the feet to the coccyx, and from the coccyx to the head. We were surprised at the results. All of the patients thus tested were about the same height. One, a man 5 feet $4\frac{3}{8}$ inches in height, after three minutes' suspension, had stretched from the feet to the coccyx $1\frac{5}{8}$ inches, and from the coccyx to the head $1\frac{1}{8}$ inches; the total increase in length being $2\frac{3}{4}$ inches. In another the total extension was $2\frac{1}{4}$ inches. In two cases there was no change in the length between the feet and the coccyx, the whole increase being between the coccyx and the head. In another case the man stretched only half an inch in all. This subject will require careful and separate study to see why it is that one man will stretch so much and another so little.

We next studied the pulse, and found that it was always increased. Before the patient is suspended, the pulse is usually excited. The following table shows the record of the pulse in different cases before and during suspension:

Before.	During.
106	144
98	130
105	155
85	100
90	120
78	107
78	105
74	107

Respiration changes relatively in about the same degree as we should expect from the change in the pulse. I shall not linger upon that point.

The results.—My results have not been so good as those reported by Dr. Stewart. There were fourteen cases under my care. There were some others under the care of my colleagues, but of these I shall not speak. Of the fourteen, four were moderately improved in one or more directions. I have arranged the results in this table, but shall not trouble you by reading it all. I have considered the condition before treatment and the result after twenty suspensions. I think that the sexual power was the condition most frequently influenced for the better. There was increase of erection, and in some cases more or less return of power, which had apparently been lost or greatly diminished before. This recalls to my mind the fact that in judicial suspensions (hanging) there is always violent erection with discharge of semen. This is, perhaps, accounted for by the violent extension of the body which occurs in that unpleasant process. In one case all of the symptoms, and notably the pain and the gait, were made worse. In other cases there was no gain at all. In four cases of locomotor ataxia there was improvement of the sexual power and increased control over the bladder. I have under observation a case of lateral sclerosis of specific origin in a man whom I have known for some years. I fancy that there is now some trouble with the posterior columns. At first there was a spastic condition, but now there is also pain. This man suffered with frequent micturition, getting up as often as twenty times during the night. The first extension made the pain worse, but cured the incontinence entirely. This was so extraordinary that, if I had not known the man well, I should have doubted it. The pain has since gotten better.

I should like to remark that we have no reports in regard to this rather promising method of treatment in early cases. It seems to me that if it is to effect a cure at all, which I doubt, it might have that effect in the very early stages of ataxia, especially if we could catch people in the pre-ataxic stage which I believe exists in a certain proportion of cases, in which there is increased knee-jerk followed by diminished or absent knee-jerk. It would be interesting to know the result of treatment in these early cases.

Dr. J. Madison Taylor: It is as yet much too early to make any deductions of value from the results achieved by the suspension treatment. At

the Infirmary for Nervous Diseases we have been using this scarce three months, and upon a total of something over twenty cases regularly so treated. It promises exceedingly well, and offers large possibilities for usefulness ; but is as yet only winning its way to our confidence. In chronic maladies of unhopeful outlook most new and picturesque remedial agencies work wonders for a time. It is essential for the honesty of our deductions to let patients clearly know that what we do should indeed aid them much, and can cause small harm, but is in the nature of an experiment. Most of the cases described by Dr. Mitchell came under my observation also. In addition to these, five patients of my own have faithfully undergone this process. To some of the results in these last I will briefly refer. In the early stages of cord troubles these effects seem best. In one case, that of a young and robust man suffering from locomotor ataxia for four months, and of distinctly specific origin, gain is clear in walking, in sexual power, and in cutaneous sensibility. The standards of comparison were carefully tested, especially in the last particular. He had been accustomed to use the faradic current with wire brush, and this soon gave increased pain. The lengthening here was seldom more, at the end of three minutes, than three-fourths of an inch from the crown of the head to the coccyx. In an older man, who has been under treatment at my hands for tabes eight years or more, and in whom the progress of the disease is arrested, the power of walking is certainly better. In a very heavy middle-aged man, weighing over two hundred pounds, the loss of flesh is remarkable, along with rapid gain in general health and lessening of special symptoms. The peculiar feature here was fall in pulse tension and frequency, from 120 to 85 standing. At first suspension was prolonged only one minute, even then it caused some cyanosis. In a case of spastic palsy due probably to a gumma of the lumbar cord little gain is noted. The hanging induces a sensation of falling to pieces amidships, and at first was followed by muscular soreness. The extent of lengthening of the body has much to do with voluntary relaxation. In well-knit muscular men it is less. In loosely-built persons it can be greatly increased by calling upon them to let themselves down. I measure from top of head to coccyx. The greatest stretch I have seen is two and three-quarters inches.

Dr. James Hendrie Lloyd: I approach the discussion of this subject with considerable diffidence, for I have not had a flattering experience with extension in spinal cord cases. I fear that this is because I have not yet given it a sufficient test. At the Philadelphia Hospital I have seen the effects of treatment on six cases. These have been suspended for three or four minutes at a time, but none of them have been suspended anything like the number of times referred to to-night. Two were cases of locomotor ataxia, one was a case of spastic ataxia, one of lateral sclerosis, and two were cases of transverse myelitis. In the case of spastic ataxia and in one case of locomotor ataxia there was slight improvement in the gait. The patients have not been able to say, from their subjective sensations, that they are

better. As far as improvement in sexual power goes I am unable to make any report.

Dr. J. P. Crozer Griffith: We have had made for the University Hospital an apparatus embodying the modifications of Dr. Mitchell, but with some slight changes. In the first place, we have omitted the smaller iron cross-bar and pulley for the support of the head; in the second place, we have not adopted the curved iron cross-bar as shown here, but have retained the straight horizontal bar, which was, I think, originally used in Russia, as is illustrated in the French papers reporting Charcot's experience with suspension. This horizontal bar is provided with a series of notches, and the advantages of these are that after the tension of the head-piece is adjusted as near as may be by the straps and buckles, it may be slightly increased or diminished by simply sliding the rings attached either outward or inward from notch to notch. Our experience with Dr. Mitchell's method of supporting from the elbows has not as yet been very extensive, as we have had the apparatus but a short time. It is, however, altogether favorable as regards the comfort of the patient.

Dr. David D. Stewart: I would merely say, in regard to the influence of extension upon the pulse, that Althaus found it diminished the number of pulsations. The pulse fell by six or eight beats per minute; he also found that the tension was increased. He inferred that this effect was produced through a beneficial interference exerted on the centres for vasomotor and cardiac action in the medulla.

Dr. S. Weir Mitchell: The number of pulsations is, of course, a point that can be readily determined, and therefore the difference in results must be due to some difference in the method employed. It may be that the pull on the head has something to do with this. I have never seen the pulse fall.

Dr. Taylor: I do not see how it is possible to determine accurately the arterial tension while a patient hangs in this apparatus, unless some other vessel than the radial be examined.

REMARKS ON THE UNPLEASANT EFFECTS OF SULPHONAL.

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[Read May 1, 1889.]

It is the almost invariable history of every newly introduced drug of real therapeutic value that, at first received with almost unanimous approbation and enthusiasm, the disadvantages which it possesses have gradually come to the fore; so that many physicians whose experience with it has chanced to be unfortunate have gone to the other extreme, and have discarded a remedy really capable of rendering great service. One has but to recall the history of cocaine to become convinced of the truth of this statement. Antipyrin, too, was and, indeed, still is used with a freedom and carelessness which the experience of many has shown to be dangerous; and the once "harmless" acetanilid now produces collapse and death. Yet these and many other of our materia medica, though capable of doing harm, are much oftener productive of the greatest good if properly used and if their possible dangers are kept in view. In like manner sulphonal came to us highly recommended by its earlier users, and with scarcely a dissenting voice concerning its admirable hypnotic action and its entire freedom from unpleasant effects. Having reported a year ago, in conjunction with Dr. Elwood R. Kirby, the result of some clinical experiments with urethan and amylene hydrate as hypnotic agents, I began with great interest and

great expectations the administration of sulphonal as a drug of the same class. Experience in a large number of cases has convinced me, however, that, though a very valuable agent, it is by no means without the power of producing undesired and quite unpleasant secondary effects; and cases now not infrequently reported in the journals confirm this opinion.

It is not at all the object of these remarks to decry a useful medicine, but to aid in establishing its true value in comparison with other hypnotics; and, by pointing out some of its possible disadvantages, to warn against the unpleasant surprises and disappointments which might otherwise lead to its abandonment. The cases to be detailed by way of illustration have occurred entirely in my own experience. Those quoted from medical literature do not at all complete the list of unfavorable cases reported in the journals.

1. First to be noticed among the disadvantages of sulphonal, and undoubtedly a very prominent one, is its *slowness of action in producing sleep.* This is to be carefully borne in mind in determining the hour of its administration; and the patient should perhaps be notified of its peculiarity, or disappointment and dissatisfaction may arise. This slowness of action has so usually been noticed in the cases under my care, that to cite instances of it would be only tedious. So generally, indeed, has the experience of clinicians agreed as to this point, that Kast, who first employed the drug, has written a second article (*Therap. Monatsh.*, July, 1888), which might almost be called a *defence* of sulphonal, and in which he discusses the reasons of this long delay in action. As a result of experiments on animals and with digestive solutions outside of the body, he advises that it be administered in a finely pulverized condition, in at least 200 c.c. of a warm fluid, with the evening meal, between seven and eight o'clock. The presence of a large quantity of fluid, of hydrochloric acid, peptones, and of salts favors the rapid absorption of the medicine.

2. Another and even greater difficulty to be overcome in the use of sulphonal is the *marked tendency which its hypnotic*

action has to persist during the succeeding day. The following case is an excellent example of this:

CASE I.—Mr. Robert S. Ulcerative endocarditis with aortic insufficiency and very troublesome insomnia. 20 grains of sulphonal given at about 10 P. M. He did not fall asleep for some hours, but throughout the whole of the next day until late in the afternoon he was so overcome by sleep and so stupid that replies could scarcely be obtained to questions addressed him, and the family became quite alarmed. The same condition was seen on another occasion after a dose of the same size. In fact, sulphonal seemed always in this case to produce sleep lasting only a few hours at night, while upon the next day the patient could scarcely rouse himself.

In such a case as this it is not pleasant on entering the room to be greeted by the friends with the words, "Oh, doctor! Those powders, those powders!"

The following less striking case illustrates the same continuance of action after a still smaller dose:

CASE II.—William S. Phthisis and chronic rheumatism. Insomnia usual, though not as a result of cough or pain. After 15 grains of sulphonal, given at 8.30 P. M., the patient slept well, but continued drowsy until noon of the following day.

The same condition is exemplified by some of the other cases to be presently related. (See Cases III, V, VI, XV, and especially XII.)

This prolonged hypnotic action has frequently been referred to by writers. L. L. Johnson (*Med. News*, September 8, 1888), for example, reports a case in which the patient promptly fell asleep after 30 grains of sulphonal given in the evening, and was very somnolent until after noon of the next day. In smaller amount the drug acted very well. Algeri (*La Riforma Medica*, No. 233, 1888) says that the action of large doses continues through the following day, and Oestreicher (*Berlin. klin. Wochenschr.*, June 18, 1888) has seen sleep remain absent during the night, but persist through the following morning. Lovegrove (*Brit. Med. Journ.*, May 26, 1888), too, states that the effects on patients to whom he gave the drug were very discouraging. For several hours after taking it no appreciable action could be noticed, but during a great part of the

next day there was extreme drowsiness and considerable cyanosis. The case of Schotten's, presently to be quoted, is of especial interest in this connection. Kast discusses this "postponed action" in the article referred to, admitting that it is often witnessed, and that it constitutes a great disadvantage when it is necessary that the patient be actively employed during the day. For this difficulty he has no special remedy to propose in addition to the method of administration described, except that the dose be carefully adapted to the individual.

3. But this *determination of the individual dose*, in order to overcome the difficulties mentioned, itself constitutes a sometimes insurmountable obstacle to the use of sulphonal. I have frequently found that the amount necessary to produce satisfactory sleep was followed by the prolonged effect on the following day, or by various unpleasant after-effects presently to be described, while any smaller amount failed to bring the desired result during the night. In quite a number of cases it was observed that a small dose merely made the patient uncomfortable in the night and upon the following day, while a full dose procured satisfying sleep, and the morning was free from prolonged drug-action or disagreeable after-effects. It would seem sometimes as though sound sleep were necessary to exhaust, so to speak, the action of the medicine. Matthes (*Centralbl. f. klin. Med.*, No. 40, 1888; *Wien. med. Blätter*, December 13, 1888), too, reports a case in which very marked disagreeable after-effects appeared to develop because the sulphonal-sleep had been much disturbed by outside causes.

The following cases are examples of the fact that a large dose is sometimes less objectionable than a small one, and are also illustrative of the difficulty in determining the proper amount to give:

Case III.—George H., aged fifteen. Rheumatoid arthritis. Troubled with nervous insomnia. 15 grains of sulphonal were almost entirely without hypnotic action, and 25 grains had but little more effect. After these smaller amounts there were short, bewildered, troubled periods of sleep throughout the night, with a tendency to talk at random; and upon the days following

the patient was drowsy and uncomfortable. 30 grains, however, produced six hours' continuous, restful sleep, entirely without unpleasant after-effects.

CASE IV.—Miss O. Hysteria; great nervousness and insomnia. Sulphonal in a dose of 30 grains, given on three successive evenings, was only productive of short, delirious dozes, and upon the mornings following there were nausea, headache, and generally unpleasant sensations; so that the patient begged that no more of the drug be given her. A further trial was made, nevertheless, and 40 grains were administered. This gave her six hours' sound, uninterrupted sleep, with no unpleasant sensations on the next day. The doses of 40 grains were continued during some weeks with great satisfaction to the patient. She later found that 20 grains were often sufficient, but if very nervous she was obliged to take the larger amount.

The dose probably most often recommended by writers is 1 to 3 grammes, and it has been repeatedly claimed that unpleasant effects only follow the administration of a dose unnecessarily large. The cases which I have described show that this is not the fact. I should say, too, that I have repeatedly, and indeed usually, commenced with a small dose, but have generally found it insufficient; and can recall but one instance in which 10 grains exerted a hypnotic action. Rosin (*Berl. klin. Wochenschr.*, June 18, 1888), who has carefully tested the size of the necessary dose, found that 1 gramme was seldom, if ever, enough to cause sleep, except in those cases in which starch, given under the name of sulphonal, had an equally good effect; and Funajoli and Raimondi (*Archiv. italiano per le mal. nervos.*, etc., 1888, xxv. 325) believe that the efficient dose for men is 4 grammes and for women 2 grammes, and that the best results are not obtained until the third night of administration. Too much attention cannot be given to the proper method of testing hypnotics, as so well described by Rosenbach (*Berlin. klin. Wochenschr.*, June 11, 1888), and to the absolute necessity of eliminating the influence of psychic impressions before concluding that sleep is due to the medicine given. The relief of pain and the production of sleep by morphine powders composed of sugar of milk is a ruse too well known to need remark. Matthes (*loc. cit.*) comments on the difficulty in determining the dose of sulpho-

nal, since it not only seems to vary greatly with different persons, but at different times with the same individual. He gave 5 grammes to one patient without any effect whatever, while in another case which he reports the drug had been used repeatedly with good results; but on one occasion a dose of only 0.5 gramme occasioned very unpleasant symptoms.

A somewhat similar instance is the following:

CASE V.—Lizzie H. Insomnia. 40 grains of sulphonal were taken on retiring on four consecutive nights. On the first three a rather restless slumber, in which the patient talked somewhat, began in one to one and a half hours; followed at about 3 to 4 A. M. by a deep sleep lasting until 8 or 9 in the morning, when she woke refreshed. On the fourth night, instead of passing into the deep, quiet sleep at three o'clock, she rose, still unconscious, walked about the room with a candle in her hand,—a rather dangerous proceeding,—and screamed at the top of her voice. As she had never been known to act in this way before, her relatives were not without reason in considering the medicine at fault, and in urging her to take no more of it.

4. *Unpleasant secondary effects* have been already alluded to in these notes as constituting one of the disadvantages of sulphonal, and some of them have been mentioned in the cases already described. Many writers have observed them, so that the claim made by the makers of the drug and by some of its users, that it is totally without disagreeable secondary action, is not substantiated. Zerner (*Wien. med. Wochenschr.*, November 10, 1888), indeed, estimates that these effects are seen in ten per cent. to twelve per cent. of all cases, and Matthes (*loc. cit.*) says that they were noted in a majority of his patients. Prominent among them may be mentioned a *condition of excitement* developing instead of the wished-for sleep. The following case illustrates this condition:

CASE VI.—Mrs. McC. Disseminated sclerosis; extreme and persistent insomnia, for which various hypnotics had been used in vain. Small amounts of sulphonal had no effect whatever. Finally, 60 grains were given on each of four consecutive nights, but the patient appeared only to become more excited and nervous, and more wakeful, if possible. On the days following the nights of administration she was drowsy and uncomfortable.

Other illustrative cases are the following:

CASE VII.—Jennie S. Neurasthenia; anæmia. Annoying insomnia was relieved nicely by paraldehyde, but 30 grains of sulphonal were absolutely

without action. 40 grains given early on two successive nights made her very excited and semi-delirious, without producing any desire for sleep until about 4 A.M. From this time until the evenings of the days following she experienced nausea, headache, and vertigo, and was so generally uncomfortable that she flatly refused to take any more of the drug.

CASE VIII.—Mrs. E. Rheumatoid arthritis; simple insomnia. 35 grains of sulphonal had a very exciting effect throughout the whole night, producing short, dreamy snatches of sleep. On the next day there was nausea and drowsiness.

CASE IX.—Mrs. D. Simple insomnia. 20 grains of sulphonal only excited the patient through the whole night, and produced no sleep whatever.

CASE X.—Tak Sing. Obstruction of the superior vena cava; persistent insomnia. 15 grains of the drug were given at 10.30 P.M., and repeated in an hour. The patient slept about half an hour after each dose, but for the remainder of the night was unusually restless.

CASE XI.—James B. Alcoholic delirium of a mild type. The patient was restless, never violent, often wanting to get out of bed, but easily restrained. He was sleeping quietly at 4 A.M., when 40 grains of sulphonal were given as a test, and in about half an hour he became very unruly and delirious. He had not acted so badly before, nor afterward until the like amount was administered on two other occasions, when the same results were observed. On another day 20 grains, given at 7.30 P.M., produced so much restlessness and wakefulness that 45 minims of amylene hydrate had to be administered at midnight, after which he went to sleep.

It is worthy of note in this case that paraldehyde, chloral, or the bromides also exerted a quieting influence; while hyoscine produced on two occasions effects similar to those of sulphonal.

One more illustration may be given.

CASE XII.—Mr. H. Mitral regurgitation, Troubled greatly with insomnia. 40 grains of sulphonal administered on each of several nights produced for some hours intense drowsiness and the desire to go to sleep, without the power to do so. The patient walked the floor, and felt "trembling" and nervous, and queried afterward whether the powders could have made him so "light-headed." After about two hours of this condition he fell asleep, and was very drowsy during all of the day following the administration of the drug. His wife, too, remarked that "the medicine had an effect on his brain."

A tendency to an excited mental condition *accompanying* but not *replacing* sleep is shown by Cases III and IV, but especially by Case X.

Temporary *delusions* after the ingestion of the drug are reported by Fürbringer (quoted by Zerner, *loc. cit.*), and may be classified in the same category with the excited mental conditions I have reported.

Nausea, dizziness, and *headache* have been already mentioned in Cases IV, VI, and VII, as effects which may follow the ingestion of sulphonal. A similar instance is as follows:

CASE XIII.—Sallie C. Mitral stenosis. 30 grains of sulphonal had no effect for several hours. On the following day the patient felt uncomfortable, and suffered from headache and dizziness. 40 grains given on another occasion produced five to six hours' sleep, but the after-effects were the same.

Matthes (*loc. cit.*) says that in some of his cases *ringing in the ears*, headache, and dizziness were experienced on the next day; and that in two instances *vomiting* occurred. Among the patients of G. W. Rachel (*N. Y. med. Presse*, November, 1888) was one in which 1.5 grammes of sulphonal were followed by fourteen hours' sleep, and later by a sense of dizziness. Otto-Dalldorf (*Deut. med. Wochenschr.*, August 23, 1888) and Rosin (*loc. cit.*) have likewise witnessed dizziness, which sometimes lasted nearly the whole of the next day. Rabbas (*Berlin. klin. Wochenschr.*, April 23, 1888) has seen vomiting on the morning after the ingestion of sulphonal, and in another patient *diarrhœa* occurred; but he is not certain that these symptoms were attributable to the action of the medicament. *Heaviness in the head* and dizziness were observed by Sachs (*N. Y. Med. Record*, October 6, 1888) in a patient who had taken two doses each of 2 grammes three hours apart. Schwalbe (*Deut. med. Wochenschr.*, June 21, 1888) has in several instances observed dizziness, headache, nausea, and in one case vomiting, after a dose of only 1 to 1.5 grammes, and S. G. Burnett (*N. Y. Med. Journ.*, March 2, 1889) has seen two instances of vomiting.

The production of a marked *sense of fatigue*, of *depression*,

and of *confusion of mind* is not infrequently referred to by authors. J. Fränkel (*Berlin. klin. Wochenschr.*, July 23, 1888) has found these symptoms so decided in a number of patients that the drug could no longer be given them; Matthes (*loc. cit.*) refers to the sense of exhaustion and depression as the secondary effects most frequently observed, and Oestreicher (*loc. cit.*) has seen them in one or two out of eighteen cases. In one of these it was very pronounced, and the patient refused to take the medicine again. Ott (*Prag. med. Wochenschr.*, October 3, 1888) refers to the sense of fatigue on waking as the chief bad symptom which the drug leaves behind it. He reports a case in which 3 grammes caused vomiting and an attack of *fainting*. The confusion of mind is exemplified in several of the cases I have reported, especially in Case XII.

In two instances I have seen full doses of sulphonal produce a *disturbance of gait*.

Case XIV.—James M. Aortic regurgitation; insomnia. 40 grains of sulphonal were followed by sleep lasting most of the night; but on the next day the patient suffered from swimming in the head, and oscillated while walking.

Case XV.—Mr. C. Plethora. Greatly troubled by insomnia. After 40 grains of the drug, given in the evening, the patient slept well, but could with difficulty rouse himself on the following morning, and felt all day as though stumbling over something.

The occurrence of a drunken, staggering gait has also been reported by Zerner (*loc. cit.*) and by Otto-Dalldorf (*loc. cit.*). Bornemann (*Deut. med. Zeitung*, November 26, 1888) reports a case of the morphia habit in which 60 grains of sulphonal were taken at about 9 P.M., and one-half of this amount at 1 A.M. As a result there developed very marked muscular incoördination with great mental depression, lasting to some extent during six days. This is a case in proof of the observation already made by Ziehen (*Arch. f. Psychiat. u. Nervenk.*, B. xx. H. 2), that severe motor incoördination and a somnolent condition are liable to appear if sulphonal be given at the same time with opium or to opium habitués. Still another case in point is that recently published by Fischer (*Neurolog. Centralbl.*,

1889, No. 7), in which the drug, given repeatedly to a morphia habitué, produced on two occasions not only ataxia of the limbs, but *indistinctness of speech.* Burnett (*loc. cit.*) mentions the occurrence of muscular incoördination after the ingestion of sulphonal. He further reports *intense thirst* and great *dryness of the mouth* produced by it in three cases.

Engelmann (*Münch. med. Wochenschr.*, October 16, 1888) details a case in which 3 grammes produced an erythematous *eruption* over the breast and inner surface of the arms, attended by considerable itching. Another instance of an eruption occurred in a case of Schotten's (*Therap. Monatsh.*, December, 1888), in which 2 grammes of sulphonal, given on each of two succeeding nights, produced but little sleep at night, but depression on the following days. After 3 grammes on the third evening the patient slept all night, but seemed completely exhausted and extremely drowsy on the next morning, and suffered from persistent headache and loss of appetite. This condition lasted four days, when improvement set in, and simultaneously an eruption exactly resembling measles appeared over the whole body. Kisch (*Berlin. klin. Wochenschr.*, No. 7, 1889) reports three interesting cases of ill-effects from sulphonal. In the first, a patient with hemiplegia from a recent apoplexy slept the entire night after taking 1 gramme of the drug. On the next morning, however, he had complete *aphasia*, and only after eight to ten hours did this slowly disappear, the patient meanwhile feeling very weak. The second patient, after taking 3 grains of sulphonal in divided doses during the night, felt wretched and exceeding languid on the following morning, and complained of great depression and of a feeling as though his senses were leaving him. The *pulse was also retarded*, beating only 38 in the minute, and this symptom did not disappear for several hours, and under the use of stimulants. The third patient, a man sixty-two years old, slept soundly after 1 gramme of the drug. On the following morning he was horrified to find that he had had a *nocturnal seminal emission*—the first for over ten years. He also felt as though stupefied during the whole day, and could not leave his bed.

Schmey (*Therap. Monatsh.*, July, 1888) details most unpleasant results following the administration of sulphonal to a case of angina pectoris from arterio-sclerosis. Nitrite of amyl had reduced the attacks to one or two a day, and amylene hydrate produced sleep very satisfactorily. As the patient complained of the taste of the latter, the author administered 2 grammes of sulphonal, soon after which *attacks of angina* came on with great violence and with only a few seconds' intermission during the entire night, without any real sleep. For the next two days the attacks were unusually frequent and severe. It is true that the occurrence of these symptoms may have been only a coincidence, and Müller (*Therap. Monatsh.*, August, 1888) claims that this is proved by the fact that in a case of arterio-sclerosis in which he used sulphonal no unfavorable action was observed. This is, of course, only negative proof. Burnett (*loc. cit.*) reports a case of arterio-sclerosis in which the patient became cyanosed and semi-comatose for twelve hours after taking it. He believes it to be contra-indicated in this condition. Lovegrove (*loc. cit.*) has also witnessed cyanosis produced by sulphonal, as has been already stated.

5. Finally, the *uncertainty of hypnotic action* is one of sulphonal's advantages—a fault which it shares more or less with all hypnotics. Cases of failure of action on the part of sulphonal are too numerous in the literature to allow of quoting from them. Among those who have been unfavorably impressed by the drug on the ground of its inefficiency may be mentioned Wilson and Hutchinson (*Med. and Surg. Reporter*, June 9, 1888), Lovegrove (*loc. cit.*), Salgo (*Wien. med. Wochenschr.*, No. 22, 1888), and Regis (*Journ. de Méd. de Bordeaux*, July 1, 1888). Several of the cases I have reported might serve as examples, but the following are perhaps still better illustrations of its occasional lack of hypnotic power, though given in full or even very large amount.

Case XVI.—Mr. W. Fracture of spine. No pain, but persistent insomnia. 30 grains of sulphonal in the evening had no effect whatever.

Case XVII.—Mrs. F. Paraplegia; persistent insomnia, and some pain. 30 grains at bedtime did not produce sleep.

CASE XVIII.—Mr. B. Cancer of face; insomnia independent of pain. Opium often used in full but not excessive doses. 40 grains of sulphonal at bedtime only made him drowsy and uncomfortable. On the next night the same dose was given and repeated at midnight. Even this large amount produced but little sleep, and the patient complained on the following day of dizziness and headache, and of feeling "queer." The same absence of beneficial action was observed after 80 grains given on another occasion.

The large majority of writers, nevertheless, pronounce favorably for sulphonal. My own experience with it has been for the most part satisfactory; for the number of cases in which no unpleasant effects have followed or supplanted the refreshing sleep obtained by its use much exceeds the number from which I have reported examples here. The instances detailed, with those reported from the experience of others, prove, however, that it is to be used with a proper appreciation of the bad results which may follow, that the time of administration is to be carefully determined, and that the dose should be accurately adapted to each individual case.

The chief disadvantages of sulphonal may, then, be recapitulated as follows: 1. Its hypnotic action usually develops very slowly. 2. This action is very liable to be prolonged throughout a greater or less part of the following day. 3. It is difficult to determine the dose which may be given with effect and with comfort in each individual case, and this dose may vary at different times in the same case. 4. The drug is liable to produce unpleasant secondary effects, which may even replace the primary hypnotic action. Chief among these are mental excitement, nausea, vomiting, dizziness, headache, languor, exhaustion, depression, and a staggering gait. These symptoms may appear either after large or after quite small doses. 5. It very often fails to exert any hypnotic action, either in any dose whatever, or in any amount which can be given with comfort to the patient.

Though I have expressed myself as in general favorably impressed with sulphonal, yet I confess to a decided preference for paraldehyde, and especially for amylene hydrate. With the latter drug my experience has been especially satisfactory,

and disagreeable results following its administration are much less frequently reported in the journals than in the case of sulphonal. The objection that its taste is disagreeable is entirely removed by giving the full dose (45 minims) in 3 capsules of 15 minims each. It is especially to be preferred when a rapid hypnotic action is desired, or where there is not time to learn by repeated trial the amount of sulphonal suited to the patient in question. In cases, however, in which the stomach is irritable, it is probable that the latter will usually be more easily tolerated.

DISCUSSION.

Dr. S. Weir Mitchell: I have seen one case in which a remarkable papular eruption was produced every time sulphonal was given. The patient, a young woman of seventeen years, is subject to epileptic attacks. She is unable to take bromides in large amounts or or in any combination without producing the condition which I have described in this College, under the name of bromic rupia, some fourteen years ago. Her skin is evidently very sensitive to drug action. In other respects the drug acted extremely well.

Dr. J. Madison Taylor: Sulphonal will probably soon fill its proper niche, useful in some cases and not in others. The most valuable added property that I have seen it exert is that of limiting sweating. I have used it in several cases of exophthalmic goître where general relaxation of the skin was marked. Here the effect was very satisfactory.

Dr. S. Solis-Cohen: I have used sulphonal in only two cases, but in one of these with a very unpleasant experience. This was a patient in the last stage of phthisis, in whom the administration of bromides and of choral produced restlessness and other disagreeable effects. 45 grains of sulphonal were given, in three doses, of 15 grains each, every two hours, beginning at 7 p. m. The first two doses were apparently without effect, but about four hours after the third a condition of active delirium resulted. The man rose out of bed, railed at imaginary foes, and threatened to kill his wife when she endeavored to soothe him. The violence, I am told, subsided in a few minutes, but wakefulness and delusions of a milder character persisted for some hours. I had no reason to suspect meningitis or other cerebral troubles. In the other case the drug did not give rise to untoward symptoms. The tendency to drowsiness on the following day, as referred to by Dr. Griffith, was present.

Dr. James H. Hutchinson: I think that the reader of the paper mentioned that the smallest dose that he had ever seen have any effect, was fifteen or twenty grains. I now have a patient who has been taking ten grains every night for some weeks past with satisfactory results. I have not had much experience with sulphonal except in hospital cases, where its action has been very satisfactory. I do not recall having seen any of the effects to which Dr. Griffith has referred.

DESCRIPTION OF HÉNOCQUE'S HEMATO-SPECTROSCOPE.

By FREDERICK P. HENRY, M.D.

[Verbal Communication, May 1, 1889.]

A friend of mine recently told me that he felt the necessity of possessing an instrument for estimating the corpuscular richness of blood, and asked me to recommend one. At the time of this request the instrument I hold in my hand was on its way across the Atlantic, but I said nothing about it for two reasons. In the first place, I had the puerile wish to be the first in this country to possess and use the instrument, and I believe this wish is now gratified; and, secondly, I acted upon the well-known Biblical principle of holding fast to that which is good. I am now satisfied that I was right in saying nothing about this apparatus, for in several important respects it is inferior to those already used for examining the blood. Had I spoken of it at the time referred to it would have been with some enthusiasm, for I had been greatly interested in the published accounts of it.

The principle on which this instrument in constructed is, from first to last, diaphanometric. The hematoscope is composed of two plates of glass of unequal thickness, which when placed in contact with each other inclose a prismatic space, of which the larger extremity has a diameter of $\frac{300}{1000}$ millimetre. The two plates of glass are movable upon each other and maintained in contact by two nickel-plated metal clasps. In the grooves formed by these clasps, the upper glass plate is freely movable upon

the lower and can be detached from it when needed for cleansing. On the lower glass plate is engraved a millimetric scale extending from 0 to 60 millimetres. The slope of the superior glass plate is at the rate of $\frac{5}{1000}$ millimetre to the millimetre, so that in order to ascertain the thickness of any part of a layer of blood contained in this cavity, it suffices to multiply the number on the corresponding division of the scale by 5. For example, the depth of the layer of blood at the widest part of the prismatic space—the right-hand extremity—is, as already stated, $5 \times 60 = 300$—*i. e.*, $\frac{300}{1000}$ millimetre; at 14 it is $5 \times 14 = 70$—*i. e.*, $\frac{70}{1000}$ millimetre, etc.

Having filled the prismatic space with blood, which is accomplished by capillary attraction, the hematoscope is placed upon this white enamelled plate which contains a millimetric scale precisely identical with that on the larger glass plate, and certain numerals beginning with 4 and ending with 15. It stands to reason that the poorer the blood is in coloring matter the greater the number of letters proceeding from left to right which will be visible. The numerals are, of course, arranged in inverse sense—*i. e.*, the higher figures to the left and the lower to the right, and this is done because the last figure distinctly seen represents the amount of hæmoglobin contained in 100 grammes of blood. For example, healthy blood contains 14 per cent. of hæmoglobin, and with a healthy specimen of blood 14 is the last number distinctly seen. With a poor specimen of blood, 13, 12, 10, 9, etc., may be visible; each numeral representing the percentage amount of hæmoglobin. Surely, nothing can be simpler in theory than this method of examining blood.

We next come to consider the application of the spectroscope to this examination, in which, for my part, I see no advantage whatever. Having filled the prismatic space with blood, it is held in the left hand and this direct vision spectroscope applied to it. Proceeding from left to right, the two absorption bands of hæmoglobin come gradually into view, and at last a point is reached where they become equally obscure; this is noted and the richness of the blood in hæmoglobin is thence determined.

This is also very simple and very beautiful in theory, but in practice I must confess to great difficulty in determining the exact point at which the two absorption bands are equally obscure—*i. e.*, with this spectroscope. With one containing a scale of wave-lengths, this difficulty is said to vanish, for the two bands are equally obscure when their extent in wave-lengths is identical.

There is a third application of this instrument which, in theory, surpasses all that has been hitherto accomplished in the examination of blood. Many years ago it was thought that accurate knowledge concerning the functional capacity of the blood could be obtained by an enumeration of its red corpuscles, and many ingenious instruments have been made to simplify this procedure. It was soon ascertained, however, that the functional value of equal numbers of red blood-corpuscles might vary within very wide limits, and this fact was thought to depend entirely upon the quantity of hæmoglobin contained in each specimen. Instruments which rivalled in ingenuity those employed for counting the corpuscles were next invented for estimating their percentage of hæmoglobin. Among these may be mentioned the instruments of Preyer, Malassez, Gowers, Fleischl, etc. It was now thought that the extreme limit of accuracy in the examination of blood had been attained. Nevertheless, exceptional cases of anæmia were now and then encountered, of which the symptoms were by no means explained by the instruments of precision to which I have referred. In a number of cases I myself have felt satisfied that the figures obtained with these instruments did not properly represent the functional capacity of the blood. The explanation of such anomalies is not far to seek. A blood may contain a normal number of red corpuscles, and each corpuscle a normal amount of hæmoglobin, and yet may be functionally imperfect. The hæmoglobin of the red corpuscles carries oxygen to the tissues, but if this oxygen is not delivered, or, in other words, if the hæmoglobin is not reduced, one of the most important functions of the blood is not performed. With this apparatus it is claimed by Hénocque that the *activity of reduc-*

tion of arterial blood in the capillary vessels may be accurately estimated. The *modus operandi* is as follows: On applying the spectroscope to the thumb nail, in a suitable light, the absorption bands of hæmoglobin are faintly visible. If a ligature be now applied to the thumb the absorption bands will shortly disappear, and the time necessary for their disappearance represents the activity of reduction.

A special spectroscope, with electric lamp attachment, has been invented by Hénocque for this kind of study of the subungual circulation.

The objections to this instrument are twofold. First, it requires six drops of blood, which is an amount too great to be obtained from the finger of a pernicious anæmic patient or of one suffering from scurvy. I speak from a considerable experience of both these conditions. Secondly, as the microscope is not employed with this apparatus, those who use it exclusively are deprived of the valuable information furnished by the size and shape of the corpuscles. Thirdly, for the same reasons, the number of white cells cannot be estimated. For these reasons I do not think it will supersede the instruments already employed for counting the corpuscles. The study of the subungual circulation is an entirely different matter, of which the value is yet to be determined. I may say, in conclusion, that it is already contested.

CHRONIC ENDARTERITIS, AND ITS CLINICAL AND PATHOLOGICAL EFFECTS.

(CHRONIC BRIGHT'S DISEASE.)

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[Read June 5, 1889.]

Chronic endarteritis is as much a disease by itself—a separate entity—as are typhoid fever and measles. It is in the highest degree important that this fact should be recognized, and that the disease should be transferred from the position it now occupies among the obscure and little understood pathological curiosities, to the rank it is sure soon to claim for itself among the common and well-known complaints of every-day life. Until clinicians ordinarily are prepared to diagnosticate it during life and to announce it as a cause of death among their patients, its frequency of occurrence and importance will not be fully perceived. The name by which a disease is called is not an unimportant matter, for, if a name be accurately descriptive it is well received, and those who hear it easily comprehend what is meant, and thereby knowledge is rapidly diffused. The name "arterio-capillary fibrosis," proposed a good many years ago by Gull and Sutton, has never been commonly accepted, for the complaint cannot justly be called a fibrosis, and disease of the capillaries has never been proved to be an essential part of the process, and therefore their description of the pathology was incorrect. The fact that our knowledge of the complaint is still most incomplete is no more reason for longer delaying to receive it

among ordinarily recognized diseases than it would have been right for Bright to withhold his great addition to the sum of pathological knowledge because there was still more to be learned in the same line of thought. Since there has come to me the feeling that I have more understanding of the pathology of the disease than formerly, the clinical management of cases often occasions a good deal of difficulty by placing me in an awkward position with my patients when they or their friends ask: "What is the disease with which they are suffering?" To say Bright's disease in every case in which albumen and casts are found in the urine would be very far from a correct expression of my understanding of the malady, for the instant conclusion of the public is that the patient has a disease which is as incurable as cancer, and one which will terminate in death almost as soon; whereas in many cases I *believe* that complete recovery, so far as the further progress of organic change is concerned, takes place, and I *know* that in many others the patients live in comfort for long periods of years. Such being the case, a name is needed by which to designate the disease, and it must be one from which the word kidney will be excluded, for it may be asserted that it is now known that the disease does not necessarily involve kidney change, and, if it does, that such change is only a very small part of the whole, and is subsidiary to something else. The pathological lesion which is always present, and which, so far as investigations made up to the present time show to be most widely diffused through the organs and tissues of the body, is an inflammatory thickening of the *intima* of the arteries and arterioles, and, whether the future will show this to be the primary and original change and the true cause or not, it is so widely diffused and so important that to call *it* the disease seems to me much more correct and descriptive than any other that has been proposed. Chronic endarteritis, a simple inflammation of the lining of the arteries, is the process by which the disease begins and thence extends to almost any degree throughout the arterial system, to some degree into the veins, and probably into the capillaries, as has been proved by the

observations of others and my own.[1] This name should be modified to meet the needs of individual cases, as the pathological lesion attacks one or another or several organs, by saying—chronic pulmonary endarteritis, chronic cerebral endarteritis, chronic cardio-renal endarteritis, or other combinations as may be required to make the name accurately descriptive of the variations of one case from another.

To illustrate somewhat the clinical aspect of the subject, a few cases will be, as briefly as possible, given to show that the disease may be recognized during the life of the patient by physical and rational signs as well or better defined than are the diagnostic features of many well-known diseases. The cases will be grouped strictly according to their clinical features and the order of succession of symptoms, showing the disease to have taken possession of one or another or several of the important organs of the body:

Case I.—A man about fifty years of age after a year or two increasing very much in weight and becoming inactive, though previously very strong, was seized in the night with copious epistaxis, a thing he had not previously had for thirty years. Examination of the urine showed the presence of a trace of albumen and very numerous casts, and a month later he had cerebral hemorrhage, of which he died in three days.

Case II.—A man about sixty-seven years of age, who had always been typically strong and healthy, was taken with severe epistaxis several times within a day or two, a thing he had not had in fifty years. Physical examination of heart and lungs was negative, but there was some stiffening of the radial arteries. There were neither casts nor albumen in the urine, but slight œdema of the ankles was present. This observation was just made, and what shape the disease will take remains for the future to decide.

Case III.—A woman of about sixty-five, very strong and well preserved, was seized with severe epistaxis; examination of the urine demonstrated the presence of albumen and casts. From this time she enjoyed quite good health except that occasionally she would be vaguely out of sorts when albumen and casts would be present in the urine, though at others absent, until she was seventy-seven, when she had slight right facial paralysis, with partial aphasia and much distress and confusion of mind, of which she was perfectly conscious. The paralysis soon passed entirely away. After this there was gradually increasing loss of memory though no perversion of

[1] The Medical Record, July 7, 1888, and Trans. College Phys. Phila., vol. x., 1888.

mental function, and at seventy-nine she was seized with sudden violent pain in the head, radiating down the back and into the limbs, particularly the lower ones; stupor came on, increasing to coma, and in a week she died.

Case IV.—A woman about fifty-nine years of age, who had always been unusually strong except that she was subject to take cold and have quite violent cough, was seized with epistaxis which recurred several times within a week or two. Examination of the urine revealed the presence of a trace of albumen, and after careful search by two observers, one of them found one or two casts. Physical examination was negative, the lung and heart sounds being natural, though when she had cold and cough, bronchial râles would be present. From that time to the present she has been very nervous, complaining of many vague sensations and having became visibly much older. She cannot walk freely and with comfort, as her steps are always short and uncertain and she tires easily. Now, two years later, there is a distinct and rather harsh aortic murmur which is systolic in time, and this, though she has never had any acute illness in the two years that have elapsed. A trace of albumen is constantly present in the urine, and occasionally a few casts.

Case V.—A man of about sixty, in the hospital ward, suffering with typical chronic Bright's disease—dropsy, albumen and casts in the urine, hydrothorax, and great hypertrophy of the heart—declared upon most careful cross-examination that the first deviation from health with which he had suffered was a dispostion to take cold readily and have violent cough, and this for some time before any of the other conditions mentioned came upon him.

Case VI.—A man about sixty-six, was for a year or two subject to violent spasmodic cough with much expectoration; he had bronchitis and some enlargement of the heart and œdema of the ankles, but neither albumen nor casts in the urine; five years later he is still living, but there has been in that time no opportunity to see or examine him.

Case VII.—A man of fifty-six, previously healthy, except that he had at times passed gravel, had bronchitis and coryza with more oppression than the physical signs seemed to account for. Examination of the urine revealed the presence of albumen and casts. For four years subsequently albumen and casts were found from time to time upon examination, but since that period there has been no opportunity to test the urine. With the exception of much rheumatic stiffness and a tendency to take cold readily he is still living and healthy at seventy-one years of age.

Case VIII.—A man, previously very healthy, had, when about sixty, an attack of pneumonia of subacute character. Since that time he has never been the same, having lost his activity and strength, and now, at sixty-seven years of age, his condition is as follows: He has lost a good deal of weight; some of the arteries are rigid; he is very easily fatigued, and complains that his legs are almost paralyzed, so great an effort is it to him to walk, though to all

appearance his gait is natural enough, except that it is rather shuffling. The heart-action is rapid and feeble. Neither albumen nor casts have been found in the urine.

CASE IX.—A woman of about sixty-five complained of not being well, though she was not confined to bed. Physical examination revealed dulness over one lung posteriorly and very harsh bronchial respiration, but no moist sounds or râles of any sort and no expectoration—plainly a case of pneumonia. There was a slight mitral valve murmur, and the urine contained albumen and casts. This condition of affairs continued, and she gradually failed, though at no time having any urgent or alarming symptoms, and after four months died very quietly.

CASE X.—A lawyer of fifty-four, in full practice, had an attack of acute catarrhal pneumonia. He was ill for several months, and during the whole time there were albumen and casts in the urine, and for between two and three years afterward. After that they disappeared, and eight years subsequent to the attack the urine was found to be healthy; since then there has been no opportunity to examine it, but the patient is still living, appearing to be healthy, and practising his profession at sixty-eight years of age.

CASE XI.—A man, of about sixty-six or sixty-seven, who had always been well, for a few months did not seem quite himself in strength, and finally had some cough. Examination revealed the presence of subacute pneumonia and albumen and casts in the urine, though there were not then nor subsequently any symptoms which could be attributed to faulty renal action. There was extreme atheroma, so much so that one radial pulse was almost extinguished and the vessel very hard. After a few weeks the lungs cleared and he got about again, though continuing very weak. In about three months there was a return of the pulmonary trouble, with cough and expectoration, and the breathing was quite rapid, though he declared he had no pain, or even oppression. In a few weeks he died, fading out very quietly, without violent or urgent symptoms of any kind.

CASE XII.—A man, who is now about eighty-four, and, for his age, quite active, has for the past fifteen years had very frequently albumen and casts in his urine, and during most of that time his breathing has been somewhat wheezing, with bronchial and asthmatic râles to be heard in the chest.

CASE XIII.—A man, who inherited gout, was, for ten or twelve years, subject to the arthritic form. When between fifty-seven and fifty-eight years of age he began to have some shortness of breath, and six months later had one night an attack of oppression so violent that it was thought he would die. Albumen and casts were found in the urine, and from this time he failed, and died at sixty years of age, having spent the last eight months of his life in the house in most shocking suffering from general dropsy and great oppression, and latterly quite insane (not delirious), being very suspicious and having marked delusions. The heart was greatly hypertrophied

and the arteries diseased (see Figs. 1 and 2), and once he had a slight uræmic convulsion.

Case XIV.—A woman, of about sixty, had for a good many months been getting less and less able to walk. Examination revealed the presence of albumen and casts in the urine, stiffening of the mitral valve, and a tumor in the right hypochondrium, which was thought to be due to gall-stones in the bladder; this, however, she had had for a good many years, and it seemed quite latent so far as giving rise to any irritation was concerned. Many of the arteries were stiff. She walked very feebly and with a shuffling gait, hardly lifting the feet at all, though there was no true paralysis. The feebleness in walking increased, and the legs wasted until the calves could almost be spanned with the thumb and forefinger, and it was with difficulty that she could even cross a room. One day she fell upon the floor, simply losing her balance, and broke the neck of one femur, and never was able to walk again. After this, and before too, her intellectual powers became less and less, she lost memory to a great degree, and at last became mildly insane, having delusions. She failed gradually, and after two years died, without having had any violent or active symptoms beyond those mentioned, except that for some time before death there was marked general hyperæsthesia.

Case XV.—A woman of about sixty-two has, during the past six years, been subject to attacks of sharp fever which prostrate her very much and keep her from one or two days to a week in bed. At these times albumen and casts have very frequently been found in the urine. She has the signs of slight mitral valve stiffening. Except when suffering with an acute attack she lives a very active life, doing a great deal of charitable work. During the last eight months she has suffered with most persistent prickling sensations (pins and needles) in the hands and feet and becomes easily fatigued, complaining that her legs are often so heavy that she can hardly lift or drag them along.

Case XVI.—A man thirty-two years of age, had a little more than two years before his death an attack of acute rheumatism, after which he had always shortness of breath and palpitation on exertion. During the last seven weeks of his life he had all the commonly accepted signs of Bright's disease—dropsy, albumen and casts in the urine, frequent micturition, cough with bloody expectoration, enormous hypertrophy of the heart with aortic regurgitation, great oppression and difficulty in breathing, violent vomiting, and pain in the belly. He died in great suffering. (See Fig. 3.)

Case XVII.—A man who during all his life has been very strong was taken at about sixty-three years of age with attacks of vertigo. Examination showed that his heart acted with the most extreme irregularity, but that there was no sign of valvular disease and the urine contained neither albumen nor casts. These symptoms continued for about one year, but during the last year, he being now about sixty-five, the condition is much improved, as he seldom has any vertigo and the heart is no longer very,

irregular though occasionally intermittent in its action. There is some stiffening of the arteries.

CASE XVIII.—An infant that was deserted by her mother when between two and three weeks old, having already before this time lost weight and failed in general appearance and condition instead of gaining, was placed in an institution. The urine was never examined. In spite of all that could be done for her she soon presented the appearances of marasmus in a most typical manner, and at last, after having for a good while looked like an old, dried-up woman, she died at five months of age. (See Fig. 4.)

These cases were selected because they presented, in a typical manner, the clinical symptoms it has been my desire to group together so that anyone who will study the subject can readily and with precision make a diagnosis of the disease. In Cases XIII, XVI, and XVIII autopsies were made, and the arteries figured in the plates were taken from them. I could have given a larger number in which autopsies were made and the lesion found, and such specimens are still in my possession, but the three only were introduced because they are fair types of the lesion and the introduction of more would have made this paper too long, especially as my desire has been to deal now more with the diagnostic and prognostic features of the disease, and to touch upon the pathology, with which I have dealt more at length elsewhere (*Trans. College of Physicians of Philadelphia*, vol. x., 1888, and *Medical Record*, July 7, 1888), only so much as was necessary to make plain my meaning, without going so deeply into pathological technicalities as to fatigue the ordinary practitioner. Only three of the cases (V, XVI, and XVIII) were hospital patients, the rest being taken from the records of private practice, and were selected because they were under observation in almost every instance for long periods.

Cases I, III, VII, X, and XII have already been alluded to in a paper which appeared in the *Transactions of the College of Physicians of Philadelphia*, vol. vi., 1882, entitled "Clinical Observations on Albuminuria, based upon a study of sixty-two cases seen in private practice."

One of the cases (XVIII) may be thought to have been improperly placed in the group, as it was one of ordinary in-

fantile wasting, or, if the name be not objectionable, marasmus, and it presented no unusual clinical features or symptoms; but it has so long seemed to me that there must be some parallelism between the disease under consideration in adults and some of the cases of infantile wasting that, upon finding the pathological lesion so typically developed (see Fig. 4), I added the case to the group; where from the pathological standpoint certainly it justly belongs. Infants dying in this way grow to look so exactly like those who die of old age that every one who has written upon the subject has remarked upon the similarity of the appearances presented, though the two modes of death are the antipodes of human existence. Upon previous occasions I have published three papers on this subject, and it will be my endeavor not to repeat unnecessarily what will be found in them. They are as follows: "Clinical Observations on Albuminuria, based upon a study of sixty-two cases seen in private practice," *Trans. College of Physicians Phila.*, vol. vi., 1882; "Cardiac Degenerations and Bright's Disease the Result of Changes in the Intima of the Arteries and Veins," *American Journal of the Medical Sciences*, June, 1888; "A Study of the Arteries and Veins in Bright's Disease," *Trans. College of Physicians Phila.*, vol. x., 1888; and the *Medical Record*, July 7, 1888.

Symptoms and Diagnosis.—The symptoms of this disease are of such a nature as, in marked cases at least, to make the diagnosis an easy matter. Perhaps these symptoms cannot be better elucidated than by passing them in review and trying to collect them together in such groups as they would seem most naturally to fall into, for to my mind certainly they present a clinical picture which is most striking and characteristic. Hemorrhage, whether it appear in the form of epistaxis or of cerebral apoplexy, is a marked symptom and one which should always rouse the physician to investigate whether endarteritis has not taken possession of some part of the vascular system of the patient. Epistaxis was the earliest symptom in the first four cases narrated to draw attention to any deviation from health in the patients. Any externally distinguishable stiffen-

ing of the arteries, commonly called atheroma, is to my mind positive evidence of the presence of endarteritis, for I have yet to examine an atheromatous artery exhibiting diseased middle and external coats and a healthy intima. Many of the commonly received symptoms of kidney disease, though they do most certainly indicate that that organ has undergone grave pathological change, causing damage to a greater or less degree, and which will be permanent and irreparable, show much more, if the warning is but heeded, that extensive and often much more dangerous degenerations are taking place in other and perhaps far-away organs and tissues of the body. Occasionally in patients affected in this way there may be from time to time attacks of fever, and at such times albumen and casts may be found in the urine, as in Case XV.

The various manifestations of disturbance of cardiac function are among the most constant accompaniments of endarteritis in any form, and as far as present knowledge leads us to a comprehension of the subject, among the most important and of greatest gravity. It has long seemed to me the merest folly in all cases of demonstrable cardiac valvular disease to set it down as caused by rheumatism or chorea, or even to over-exertion of the body, as is so commonly done by clinicians, or, on the other hand, to believe that rheumatism commonly induces endocarditis and stops at that. Cases IV and XVI are types of what may happen in these respects, and as they afford positive evidence of what sometimes does take place, they are of more value than many negative cases, for it is an axiom in logic that it is hard to prove a negative. In Case IV the patient had no cardiac valvular disease two years ago, now she has positive evidence of aortic stiffening, and yet during that time she has never had any acute attack of illness, but has most positive signs of endarterial inflammation—stiffening of the superficial arteries, albumen and casts in the urine, the valvular heart disease, great nervousness, with loss of muscular power, and general rapid aging in every respect, all coming on after a violent series of attacks of epistaxis which ushered in the loss of health. Most certainly the valve change was not

due to rheumatism or chorea or overexertion of the body in this case, for no one of these possible causes has obtained, and if it be not granted that the inflammation that certainly has existed in the lining of the arteries has crept slowly and silently into the heart, there to accomplish the same fell work that it has undoubtedly done in the vessels, no other more reasonable explanation at any rate can be offered. The history in Case XVI seems equally positively to show that the effects of rheumatism by no means stop after having injured the endocardium and valves, for in that case, from the time of the acute attack, the patient was no longer well, and in about two years died of Bright's disease, with a typical development of the symptoms and pathological lesions (see Fig. 3). Looking at the case from my own standpoint, I feel quite satisfied that the man had rheumatic endocarditis, and at the same time or soon after, and as a continuation of the same process of disease, endarteritis, at first acute and then continuing in a chronic form after he seemed well, and that by this avenue and in this way almost all his organs became gradually involved. It would be easy to accept the old mode of explanation—rheumatic endocarditis, aortic regurgitation, cardiac hypertrophy, increased arterial tension, hypertrophy of the muscular coat of the arterioles, venous congestion of the kidneys, and thus Bright's disease secondary to the rheumatic heart disease. This as an explanation has long seemed to me more than merely theoretical—it is positively fanciful and visionary, and besides, it totally fails to explain many of the most evident and important of the pathological lesions. For the sake of argument, suppose we accept the hypothesis as affording an adequate explanation of the lesions so far as the heart and kidneys are concerned, how are the diseased conditions of the other organs to be explained? The lungs were much infiltrated with cellular overgrowth and their vessels were markedly thickened, especially the intima, the liver in a state of granular disorganization, the bloodvessels of the spleen very thick, and the aorta (though the man was only thirty-two) very atheromatous, beside which the heart exhibited fibroid degeneration and its arteries were

in the last stages of degeneration, not hypertrophy (see Fig. 3). As an explanation for all this is it not more easy to believe, and more consonant, too, with our knowledge of the migratory habits of inflammation, that an endarteritis, rheumatic in origin, acute at first, and then continuing as a chronic process, took possession of the lining of the arteries, and creeping from place to place by avenues so widely open as actually to invite invasion, thus gradually produced the result, than to distract our brains by trying to follow the intricacies of the older explanation, which is so complicated that from the time it was originally proposed it has always been looked upon with suspicion?

Œdema coming on without discoverable organic lesion sufficient to account for its presence is often an indication of the existence of endarteritis (Case II), and the same may be said of vertigo and irregular heart action in some instances (Case XVII).

The fact that symptoms connected with pulmonary disorder or actual lung diseases are often the earliest departures from health in those who afterward die of what has been called chronic Bright's disease, was one of the things which first caused me to feel any doubt of the sufficiency of that mode of explanation of the train of symptoms. Among the cases detailed this is plainly illustrated, for in eight at least, Cases V to XII inclusive, the mode of origin was an attack of pulmonary disease, and six of them certainly were cases that every one would agree to call Bright's disease. This phase of the complaint may declare itself in the form of an attack of bronchitis, either by cough and expectoration without physical signs, or with a full development of bronchial râles, as the case may be, or of catarrhal pneumonia, a disease which is capable of taking upon itself very many and varying aspects, or of asthma or emphysema; or, lastly, there may be one of the forms of oppression, either sudden and violent, or coming on gradually and being very persistent, which are such common and well-known accompaniments of chronic disease of the kidney. These manifestations of pulmonary disorder may be acute and accompanied by great fever

and general disturbance of the system of such a character as to constitute an acute illness confining the patient to bed, or their onset may be so insidious as to attract little or no attention until a physician discovers that his patient is going about with a lung half solid from pneumonia (Cases IX and XI).

Functional disturbance of the liver is not uncommon, and the frequency with which the bloodvessels are thickened shows it to be likely that it has here too some causative relation. The spleen is incapable of inducing clinical symptoms of which at the present time we have knowledge, but it is one of the organs in which the pathological change of the vessels is most commonly found.

Symptoms manifesting a disordered state of the nervous system have long been well recognized as a common consequence of this disease, and of late several interesting and valuable communications upon the subject have appeared in different medical journals,[1] but the earliest attempt probably to arrive at a scientific understanding of their cause was made in the paper of Gull and Sutton.[2] In these symptoms of disordered nervous function do we find what affords perhaps the strongest confirmatory evidence that the disease has its origin in the vascular system, and at the same time the best proof that the old theory of origin is totally untenable. There has never been even an attempt made to explain the cerebral apoplexy and many other less formidable symptoms of nervous disorder that are so common, and clinically so fully recognized, except in a general way to allude to the many things that increased arterial tension is capable of effecting. The most common of these symptoms—as has already been somewhat brought out in the detail of cases given—are: cerebral apoplexy, general muscular weakness, with shuffling gait but without actual paralysis, and, in some instances, wasting of the

[1] Chronic Bright's Disease (Arterio-capillary Fibrosis) in its Relations to Insanity. By E. A. Christian, M.D. The Journal of the American Medical Association, March 23, 1889.

[2] On Changes in the Spinal Cord and its Vessels in Arterio-capillary Fibrosis. Trans. Path. Soc. London, vol. xxviii., 1887.

muscles; change of temper and disposition, nervousness and decline of mental power, especially loss of memory, and even mild insanity; and in old people in the latter stages of the disease, most commonly after they have got so weak as to be confined to bed, a curious general hyperæsthesia which does not seem to be confined to the skin, for there is great pain caused by any movement.

Rheumatism and gout seem to be near relatives of endarteritis, or at least closely bound up with it. In one of the cases (Case XVI) rheumatism, and in another (Case XIII) gout, seemed to set fire to the train that finally led to the fatal result, and in both, as has already been alluded to, post-mortem examinations were made, and the lesions of chronic endarteritis in a typical state of development found (see Figs. 1, 2, and 3), and they both died with all the ordinarily recognized symptoms of chronic Bright's disease.

Prognosis.—In the clinical management of cases there is nothing which occasions more difficulty than the question of prognosis; for, to any one who has had to deal much with the disease, one thing at any rate is very plain, that many cases that a few years ago would have been looked on as necessarily fatal, recover entirely, and others regain sufficient health to live for many years in comfort. It is wonderful how differently two cases, that at the outset seemed very closely parallel, may terminate, the one perhaps living for many years, and the other dying within a few months; this is exemplified in Cases XI and XII. It does not seem possible to doubt that in Case X, for instance, entire recovery has taken place, though, as albumen and casts were present in the urine and the patient was in ill-health for more than two years, he certainly had chronic Bright's disease; and Case VII is one in which, even if it cannot be positively asserted that complete recovery has occurred, at any rate there has been such a physical condition that for fourteen years the man has had a very enjoyable life. It seems possible to explain this, so far as the kidney is concerned, only by the belief that there was, at the time of the acute attack, a wedge-shaped spot or spots of inflammation with

round-cell infiltration, the broad base of the wedge being toward the cortex, such as are so commonly seen in sections cut from the organs of persons dying in this stage of the disease, and that this has in the course of time healed, after having destroyed a greater or less number of tubules, and left in their place a small scar. Why it should so long have been supposed that the kidney was incapable of getting over the effects of an inflammatory process which is in no wise different from what occurs elsewhere, when it is perfectly well known that every other organ and tissue in the body can undergo regeneration to a greater or less degree, is a thing which is incomprehensible! In view of these facts we should, as physicians, be very slow to give a fatal prognosis, except in cases in which the morbid changes have progressed so far as to make our ground very certain. There is no combination so bad, and none which so fully seems to justify a very gloomy prognosis, as the presence of albumen and casts in the urine, with general dropsy and a heart evidently greatly enlarged, with much shortness of breath. Of these symptoms, to individualize them, there is none which has anything like the grave significance of enlargement of the heart with heaving impulse and constant shortness of breath; people with this combination seldom live more than a few months. Experience has long since taught me to believe that the presence or absence of the signs of valvular change is, comparatively speaking, of very secondary importance, and should have little influence in determining our decision of the probable duration of life, but that all our efforts should rather be turned to endeavor to form an estimate of the size of the heart and state of its walls.

Treatment.—It would make this paper much too long if any attempt was made elaborately to discuss the treatment; beside which, the question does not properly come under its scope as defined in the title. It need, therefore, only be said that the disease is so widespread in its results and produces such varying effects, as the morbid process takes possession of one or another part of the organism, that no special plan of treatment could be laid down which would cover all the dif-

ferent aspects it may present. Drugs which have seemed to me to be capable of effecting much good in certain phases of the complaint are carbonate of ammonium with small doses of digitalis—these should be given three or four times a day and their use persisted in for long periods, often for as much as a year or more. The advantages of this mode of treatment were exemplified in Case XVII.

Pathology.—As was stated, the pathology of this disease has been discussed by me in papers already published, and time fails, nor would it perhaps be desirable to go fully over the subject again. There are, however, a few points to which allusion must be made. To my mind it seems established as a fact and beyond the reach of doubt that many cases of chronic Bright's disease, if not most of them, are in truth cases of arterial disease; this was years ago asserted by Gull and Sutton; now, as a result of more recent investigation, I believe it to be established that endarterial inflammation is an important part of the disease. In reaching this conclusion one more step has been made in the direction of a final understanding of the process, though whether we are now near to this goal or still far away is a question which the future only can determine.

It would be easy, but of no present advantage, to speculate upon whether the endarterial inflammation is but an extension of disease from the blood, the living and moving tissue with which the lining of the arteries is always in contact, or whether the process starts in the connective tissues, in which the arteries lie and of which they are in fact a part, or whether again the inflammation has its true origin in the arterial intima. The probability is that the future will show the arterial inflammation to be but an extension and consequence of some other preëxisting change. Outside of the assertion that the disease begins in the blood—and this is a theory which though exceedingly plausible and not unlikely to be correct, is without a single fact of observation to establish its truth, so little understanding have we at the present time of the blood or its diseases—there has as yet been but one hypothesis offered to explain

an earlier beginning of the disease. This hypothesis, for hypothesis only at present has it a right to be called, was mentioned in one of my earlier papers (loc. cit.), and is that which was in the first instance propounded by Drs. Da Costa and Longstreth[1] and has been further elaborated by Dr. Da Costa in a later paper.[2] These authors, after making post-mortem investigations both macroscopic and microscopic, assert that there is constantly present a lesion of the renal nerve plexus, and Dr. Da Costa in his later paper states that lesions were also found in the cardiac ganglia in cases of Bright's disease. These investigations are of the highest scientific interest and exceedingly suggestive, for from a purely theoretical point of view it is fully as reasonable to suppose that the morbid process might begin in the nervous system as in the lining of the arteries or elsewhere. Investigation of the histological condition of the nerve trunks and ganglia of the sympathetic system offers a field of the greatest interest, but at the same time great difficulty, for, if the truth be told, but little is really known at the present time of its ordinary condition in human adults—that is to say, few if any pathologists have been in the habit of examining the sympathetic so frequently as to be very familiar with its ordinary condition. Further study of this matter is much to be desired, but even if such investigation should demonstrate that a lesion of the sympathetic is as constantly present as the endarteritis, it would still remain an open question which, if either, was the causative lesion.

A point of great pathological significance, and to which allusion has already been made, is that valvular disease of the heart, as recognized by the ordinary clinical signs, may come on in the course of this disease without the patient's having at any time an acute attack of illness, and this is proved by what took place in Case IV, the woman developing valvular heart lesion in the course of an attack of mild chronic Bright's dis-

[1] Researches on the State of the Ganglionic Centres in Bright's Disease. American Journal of the Medical Sciences, July, 1880.

[2] The Middleton-Goldsmith Lecture on the Relation of the Diseases of the Kidney, especially the Bright's Diseases, to Diseases of the Heart. Medical News, May 5, 1888.

ease lasting two years, during which time she had no severe illness or febrile attack—at the beginning being free from valve change, and now having its clinical evidences most unmistakably developed; showing, it seems to me, most positively that the endarterial inflammation has crept into the heart to twist and distort the valve.

It is a curious fact, and one worth recording, though at present I can offer no explanation nor draw any deductions from it, that in several different sorts of tissue removed by operation by surgeons as incurably diseased, I have found upon microscopic examination precisely the same endarterial inflammation and thickening, in some vessels so great as entirely to close their calibre, as found almost universally throughout the body in the cases of chronic endarteritis or chronic Bright's disease that have been described. These have been ovaries and Fallopian tubes removed by gynecologists, a vulvo-vaginal retention cyst, a cancerous uterus, and, I think, other tissues.

The relation of old age to this process of chronic endarteritis, or Bright's disease, I have discussed in former papers, and it does not seem possible longer to doubt that the conditions are in many respects parallel if not identical, and a further confirmation of this view is afforded by the observation I have recorded that a similar condition of the lining of arteries has been found in the heart of an infant dying of marasmus at five months of age (see Fig. 4). The arteries of young infants, just as all the rest of their tissues, present histological appearances which are different from those of adult life, and they may offer, therefore, perhaps an opportunity to learn the true origin of this process of atheroma, as it has been called, which could never be had from the study of adult tissues. The plicated membrane or fenestrated membrane of Henle presents itself in the arteries of young infants, when examined in cross-section, as a folded line somewhat thicker relatively than it is in the tissues of older persons, and in healthy vessels appears to be entirely structureless, containing no nuclei which will take stain; in the diseased artery from

the heart it seems as if this had, owing to some irritation, become inflamed, and there are many proliferated nuclei in the membrane itself, or lying inside of it, producing swellings in the lining of the artery, just as is so commonly seen in the atheromatous processes of adult life (see Fig. 4). It is impossible to avoid drawing a parallel between these appearances and what is so well known and has been so much studied in inflammation of the cornea.

The drawings were made for me by Dr. B. Alexander Randall, and they admirably represent the appearances intended to be shown. As they were to be reproduced by photographic process, they were necessarily made somewhat diagrammatic, for many of the finer lines and shadings in a drawing cannot have justice done them by the photographic method as here applied.

It was my desire in this paper to deal more with the clinical than the pathological side of the subject—to try to formulate the symptoms so that a diagnosis may be more easily made; and last, though by no means least, it is high time to give the disease a name, so that it may be commonly recognized and admitted to our every-day classifications of diseases. To me it seems plain enough, in the present state of clinical and pathological knowledge, that the morbid process is "chronic endarteritis"—and so the disease should be named.

Fig. 1.

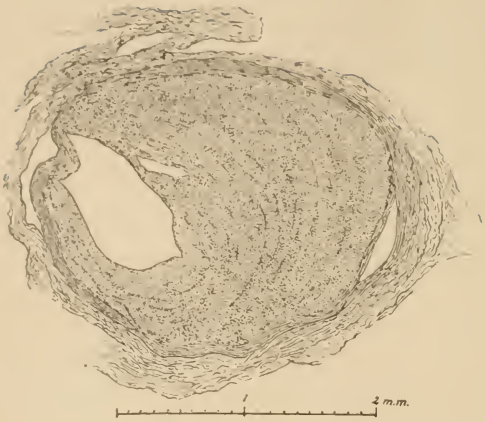
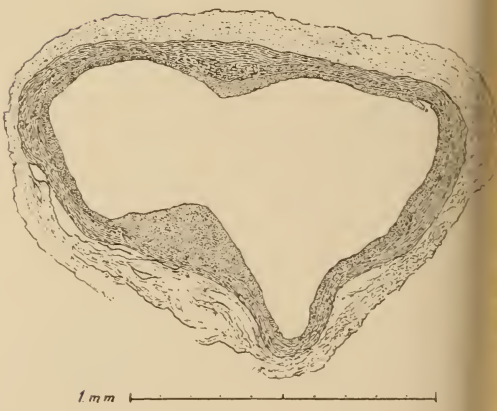


Fig. 2.

Fig. 3.

Fig. 4.



EXPLANATION OF PLATES.

Fig. 1, × 17 diameters. Is from Case XIII, and represents the large branch of the left coronary artery of the heart, which descends in the interventricular septum. The calibre of the vessel is almost closed by overgrowth of the intima, the opening which remains being represented toward the left in the picture. The other spaces were either the result of tearing in mounting, or were filled with some material which did not stain and was transparent. The muscular coat is represented by lines, and the adventitia by lighter ones outside; all the material inside these, represented by dots, is the proliferated intima. The part of the vessel represented here contained no calcareous material, though elsewhere there was abundant mineral deposit.

Fig. 2, × 9 diameters. Is also from Case XIII, and represents one of the first branches of the renal artery as it entered the hilus of the kidney. The great and irregular thickening of the intima is very marked, and the plicated membrane is distinctly visible around the greater part of the circuit forming the boundary between the intima and muscular coat.

Fig. 3, × 10 diameters. From Case XVI, and representing also the branch of the left coronary artery of the heart which descends in the interventricular septum. The enormous and irregular thickening of the intima is very evident; the plicated membrane is distinctly visible around half the circuit upon the side of least thickening; upon the same side also the muscular coat is nearly twice as thick as upon the other. There are two explanations for this irregularity of thickness of the muscular coat: the one, that it is hypertrophied upon the side on which it is thickest; the other, that upon the opposite side it has been partially destroyed by the invasion of disease.

Fig. 4, × 40 diameters. The infant was five months old. This is the left coronary artery of the heart just after its origin in the aorta, from Case XVIII. The irregularity of thickening is very evident, and it seems equally positive that the process of disease belongs to the lining of the vessel, for the muscular coat is distinctly visible outside of the area of disease, and does not appear to be very much altered, except that in some places the process seems to have invaded it as well as having produced masses inside the vessel. The plicated membrane is represented by two fine lines with a white space between them, and is visible around the entire circuit, except in the thickening which is represented in the lower part of the picture, where the process of disease has involved the muscular layers to a great extent.

DISCUSSION.

Dr. J. M. Da Costa: I want to call attention to the value of this paper, not only as a very suggestive one with reference to Bright's disease, but as touching upon some of the most important general points of pathological research. I am convinced that the time has fully come for dropping the name Bright's disease and substituting one which shows that it is a much more general condition than we associate with the idea of an affection of the kidney.

There is, however, one important practical point which appears to me not to have been brought out in this extremely instructive paper, and that is, the distinction between the different forms of Bright's disease. I believe that the author has had especially in mind the form of Bright's disease which we call contracted kidney. I think what he has said is more applicable to this form of Bright's disease than to its other varieties. Indeed, having also pursued the line of research which Dr. Meigs has described, I am by no means sure that these remarks in regard to endarteritis will apply to acute Bright's disease or to some of the other forms of affection which, for convenience, we class together as Bright's disease. I think, therefore, that in future observations it will be extremely desirable to specify the varieties of renal disease which are associated with the arterial changes, and the kind of these changes in each variety.

With reference to the name, that is one of the most difficult things to be met. Sometimes it has occurred to me that the term vaso-renal sclerosis might give an idea of the vascular conditions and also of the renal condition. There again, I have particularly in mind the contracted kidney. Still I can myself see objections to the term.

The great value of papers like this is that they teach us to look away from individual organs to the general condition. This is not only of diagnostic and prognostic importance, but ultimately will prove of therapeutic value. In truth, I am fully convinced that whatever views we may hold of the starting-point of the malady, the time has come for the recognition that it is a general rather than a local affection.

Dr. H. C. Wood: It seems to me that we shall get light upon this subject if we look upon it from an etiological as well as from a pathological point of view. I am much inclined to believe that there are two distinct diseases, either of which may be the cause of the other, and each of which may rest upon a common cause. In other words, the condition known as contracted kidney may precede the endarteritis and be the cause of that condition, or the contracted kidney may depend upon the same cause which afterward produces the endarteritis, or possibly the endarteritis may be the cause of the contracted kidney. I am sure that it is essential that we study

these cases etiologically more closely than has been done in the able paper read to-night, if we are to arrive at correct conclusions. To my mind, it is very probable that the baby spoken of died of inherited syphilis and that the child had an endarteritis which was syphilitic and had little or no relation with the endarteritis commonly seen in advanced renal disease.

Then we find that contracted kidney is often the result of the presence of a poison. It certainly can be produced by arsenic, as well as by lead. It is often the outcome of the gouty or rheumatic poison. Certainly lead may produce endarteritis as well as contracted kidney. I believe that it may produce endarteritis without contracted kidney, or contracted kidney without endarteritis, or it may produce both. I think that is true of other poisons. The lead, the arsenic, or the gouty poison circulating in the blood acts as an irritant. Now it excites a general inflammation of the walls of the blood-vessels; now it excites inflammation in the kidney. The two processes are parallel; often I think the outcome of the same acting cause, rather than dependent one upon the other.

There is another point in regard to the practical aspect of this subject. In none of the cases reported is the specific gravity of the urine spoken of. I believe myself that noting the specific gravity is of more importance from a practical point of view than noting whether or not albumen is present. If there is a persistent low specific gravity accompanied by some of the general symptoms of failure, you have the elements of a grave prognosis. In making a prognosis the specific gravity of the urine should receive more attention than is commonly given to it.

Dr. F. P. Henry: I have been much interested in Dr. Meigs's paper, which, as all here are probably aware, is a continuation of his studies of this important subject. Those who may have studied it much less thoroughly than Dr. Meigs are fully prepared to admit the association between endarteritis and contracted kidney, and believe it to be in the nature of cause and effect. This association, however, is not limited to the kidney. In an elaborate article in the *Revue de Médecine*, 1886, Hippolyte Martin contends that sclerotic processes in general, whether in the heart, the kidneys, the lungs, the liver, the spleen, the gastro-intestinal canal, or the cerebro-spinal system, are consecutive to *endarteritis obliterans*. Only with reference to the sclerotic process of locomotor ataxia he admits that this sequence or dependence is not demonstrated. The inherent difficulties of the investigation of this latter question are great, but the lines having been pointed out, it is reasonable to expect that they will be overcome. The problem, to quote Martin, consists in the demonstration whether the different columns of the cord which, in the fœtus, are developed successively, have, in the adult, an independent circulation.

I am surprised that there has been so little reference to the literature of the subject. I do not refer to the pioneer contributions of Gull and Sutton,

but to quite recent literature in which the same doctrines are advocated as have been upheld this evening by Dr. Meigs.

Dr. Henry F. Formad: I have made and studied numerous sections of kidneys during the last year, not so much in the line of study that Dr. Meigs pursued, but for other purposes. I have been more forcibly impressed with the fact than ever before, that we have a peri-arteritis where we have endarteritis. Probably Dr. Meigs has not attached so much importance to this. There are three distinct forms, which, however, I have not sufficiently studied to draw any definite lines. We certainly have predominant endo-arteritis in many cases of cirrhotic kidney, and in the fatty and contracted organ. In other cirrhotic kidneys, in fact in the majority of cases of either form of Bright's disease, there is only peri-arteritis so far as I can discover. Then we have a combination of the two.

One point that I have noticed in studying the clinical histories of the patients is, that the kidneys from syphilitic cases present predominant peri-arteritis. Syphilis affecting the smaller vessels affects the adventitia predominantly. Large arteries show more endo-arteritis. Where there is any other cause than syphilis the peri-arteritis appears to be less marked.

I think that the idea of Dr. Meigs to bring this matter of arterial changes under a general head is a good one. These certainly lie at the bottom of a great many diseases which are known by various names.

Dr. Arthur V. Meigs: What I have to say I shall endeavor to make as brief as possible. With reference to Dr. Wood's criticism, that the specific gravity of the urine should have been given in my detail of cases, I would say that it was omitted for the sake of brevity, as in most instances the disease was beyond question Bright's disease, but I have records of the point in my notes.

I must protest against what has been said of syphilitic lesions. The habit, which is a common one, of attributing to syphilis all cellular growths within arteries which narrow their calibre, I look upon as a positive obstruction to progress in knowledge. It is not reasonable to pronounce such a lesion syphilitic, unless there is a history that the patient has had the disease, or some other evidence of its existence, especially as it is now well known that the lesion does arise from other causes. As to the infant whose case I reported, it is, of course, impossible to say that the growth in the artery was *not* syphilitic, for the child was a foundling, and, therefore, nothing was known of its parents; still the conclusion that the growth *was* syphilitic seem to me equally unwarrantable, as it is well known that precisely similar histological appearances do arise which are not syphilitic. Gowers, in his lectures on syphilis and the nervous system, lately published in the *Lancet*, positively asserts that there is no histological appearance which is peculiar to the disease.

So far as concerns peri-arteritis, it goes without saying that it and disease of the muscular coat are very common—that to which I wished to call atten-

tion is the fact that arteritis is equally common, and more so, I believe; and that it is in the intima, probably, that the disease has its origin, and not, as has been commonly asserted, in the adventitia or muscular coat. From the examination alone of an artery far gone in atheromatous change, it would be impossible to say where the process had its origin, for all the coats are diseased, and perhaps equally so; but in the earlier stages of the process, before the fenestrated membrane of Henle has been destroyed, it is often found that the disease process exists only within this line, and as it constitutes an absolute and easily recognizable boundary between the muscular coat and intima, it is easy in such cases to say that all disease within it is of the intima.

It has been my endeavor, as I said in my paper, as much as possible to avoid speculation, and merely to record my observations, for in this way only does it seem likely that we shall arrive at a more complete understanding of the disease. Our present knowledge was obtained in this way:—formerly physicians saw cases that they diagnosticated as dropsy, then it was learned that with dropsy there was commonly associated what was called "coagulable urine." Bright observed that in such cases the kidneys were commonly diseased; and, further, that hypertrophy of the heart often existed; then Gull and Sutton pointed out what they called "arterio-capillary fibrosis." Now I maintain that endarteritis is as much a part of the disease. Only by the addition of further links to the chain can we ever hope to reach a final understanding of the process, and the next step, so far as I can at present see, is to follow the line of observation pointed out by Drs. Da Costa and Longstreth, and endeavor to learn what influence or association the nervous system has in the disease.

A CASE IN WHICH FRESH BONE CHIPS WERE USED TO FILL UP A LARGE GAP IN THE SKULL AFTER TREPHINING.

By W. BARTON HOPKINS, M.D.,
SURGEON TO THE EPISCOPAL HOSPITAL.

[Read June 5, 1889.]

This case is presented this evening as an example of bone-grafting in its application to trephining:

The patient, a boy twelve years of age, was admitted to the Episcopal Hospital on the 6th of December, 1888, having been struck by a freight car. His head and face had been severely dealt with. Besides numerous extensive lacerations of his brows, forehead, and scalp, he had a long, indented, compound fracture of the skull over the left parietal region, evidently caused by a right-angled bit of iron ; the depressed portion was firmly impacted. The boy showed no signs of cerebral injury beyond slight restlessness. The crown of a small trephine was applied to the posterior angle of the fracture, and the remaining portion of the depressed bone was gnawed away with the trephining forceps. The button in being removed was allowed to drop upon the floor, and was therefore discarded. All the chips removed by the forceps, however, were immediately placed in a warm carbolic solution. The dura mater was found to be uninjured, and the brain to pulsate normally beneath. A thin film of tissue corresponding in size to the gap was shaved from beneath the flap of one of the large scalp wounds and placed as a protective over the exposed dura mater. Over this was carefully spread sufficient bone—which had been finely divided by forceps into particles the size of coffee-grounds—to fill up the gap, which was two and a half inches in length by one inch in breadth, to a level with the surrounding skull. The wound was closed and drained with catgut, having been treated antiseptically throughout. The boy had no bad symptoms; the dressing was removed for the first time on the twenty-fourth day, and the wounds were all found to have healed. The boy was discharged from the hospital January 25, 1889. At this date, six

months subsequent to the injury, a very favorable condition of affairs will be found to exist. Though there is some depression in the scalp over the site of the injury, no pulsation can be detected, and there can be no doubt that a thin plate of bone has formed, which completely protects the brain. No tenderness on pressure exists, and the boy is in the enjoyment of perfect health.

This method of restoring the continuity of the skull after trephining would seem to compare favorably with the others which have been employed, as perhaps giving equally good protection to the brain, and at the same time being less liable to be followed by necrosis. Should some of the grafts fail to take, they would likely be shed in the discharge, and then, failure of the procedure would not require opening up the scalp at a remote period, as has sometimes become necessary after the reinsertion of buttons of bone or large fragments.

EXHIBITION OF AN IMPROVED APPARATUS FOR THE THERAPEUTIC USE OF COMPRESSED AND RAREFIED AIR, WITH REMARKS ON THE HOME-TREATMENT OF PULMONARY AFFECTIONS.

By SOLOMON SOLIS-COHEN, A.M., M.D.,
PROFESSOR OF CLINICAL MEDICINE AND APPLIED THERAPEUTICS, PHILADELPHIA POLYCLINIC AND COLLEGE FOR GRADUATES IN MEDICINE; CONSULTING PHYSICIAN TO THE JEWISH HOSPITAL; ETC.

[Read October 2, 1889.]

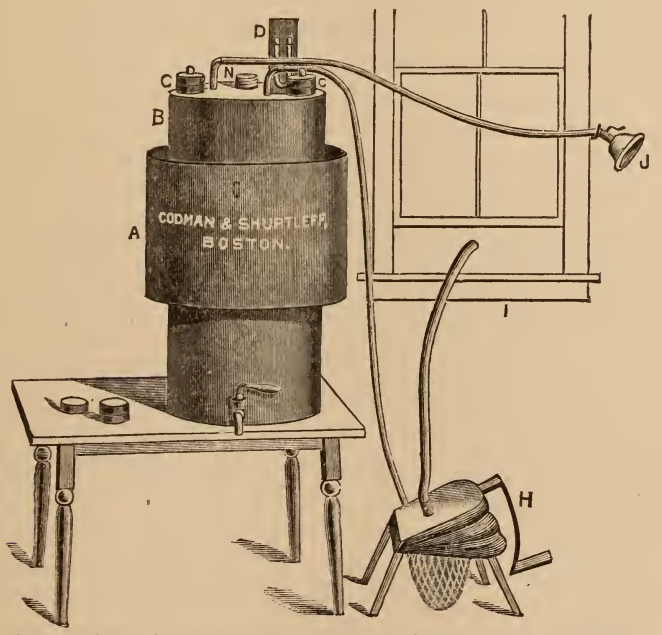
The apparatus which I have the honor to exhibit to the Fellows of the College is primarily designed for use at the home of the patient; though the same features of construction which enable it to fulfil this purpose, likewise recommend it for use at the physician's office; and by multiplying the number of delivery-tubes and introducing a motor and reservoir it may be adapted to use in hospitals. In the *New York Medical Journal* of February 23, 1889, I have illustrated the arrangement of an apparatus for hospitals, permitting the treatment of seventy patients in the course of about two hours. The apparatus for inhalation of compressed air (Fig. 1) has been exhibited previously, though not before this body. But the apparatus for expiration into rarefied air has not yet been publicly described.

My attention had been called to the value of modifications of respiratory pressures in the treatment of pulmonary affections, and especially of phthisis, even before I became a student of medicine, by some rather remarkable recoveries ensuing upon such treatment, in the practice of my brother. The apparatus which he employed was that of Waldenburg. A description of this instrument, by Dr. Pepper, and remarks upon its therapeutic employment, by my

brother, Dr. J. Solis-Cohen, may be found in the *Transactions* of the College for 1876.

Much of the success of this therapeutic measure depends upon its regular and continuous use. As it is often impracticable to accomplish this at the physician's office, the apparatus must be placed in the patient's home. Waldenburg's apparatus is large and cumber-

Fig. 1.



Original form of Apparatus for Inhalation of Compressed Air (1883).
A. Water-chamber. B. Air-chamber. C. Weights. D. Gauge. H. Bellows. I. Window. J. Mask. N. Cap for hook and sponge.

some. It is not continuous in action. Its adjustment is troublesome, and its cost, fifteen years ago, was excessive. These considerations induced my brother to undertake some experiments with the view of providing a substitute, which should be small, cheap, easily managed by a patient or nurse, and reliable as well as cleanly. He, however, abandoned the gasometer plan, and his efforts were not entirely successful. Taking the matter up where he had left off, I soon realized that the only available method of securing a certain,

regular, and continuous pressure was to return to the gasometer; but I found that the desired reduction in size could be accomplished by using the gasometer as a regulator only, and not as a reservoir. This necessitated the introduction of another source of air-supply, and as the simplest, cheapest, and most practicable, I choose the foot-bellows of the dentist's blowpipe. Any form of pump available under the particular conditions, or even city water pressure might also be adapted, but the bellows is preferable under ordinary circumstances. This combination of foot-bellows and small gasometer solved the problem so far as the inhalation of compressed air is concerned. With the assistance of Mr. Charles Richardson, an apparatus was constructed in 1883 (Fig. 1), which has since been in continuous use by others as well as by myself, and has given complete satisfaction. Though it has already been described in print[1] I may be pardoned for again exhibiting its construction, as several mechanical improvements have been introduced that have not yet been published.

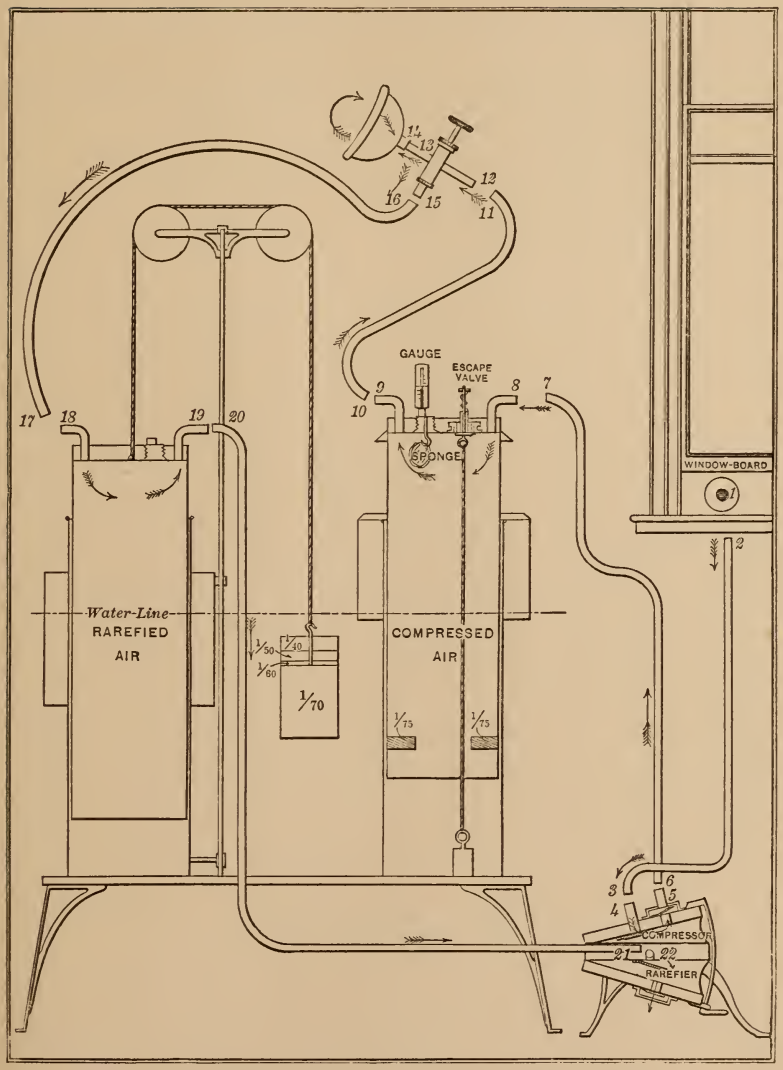
It consists of a gasometer ("Compressed Air," Fig. 2),of which the air-chamber is eight inches in diameter and twenty-four inches high. The water-chamber is pierced at the level of the base of the overflow tank (seven inches from the top), with a row of perforations, allowing the water to escape into the tank under pressure of air in the air-cylinder. The air-cylinder carries, two inches[2] from its open base, two shelves, one on each side, on which are placed ballast-weights for the purpose of lowering the centre of gravity, and thus maintaining the steadiness of the apparatus. Both shelves and weights are perforated to avoid resistance of water. As the area of the top of the air-chamber is just fifty square inches, atmospheric pressure upon it equals, in round numbers, 750 pounds. With the ballast upon its shelves the cylinder weighs ten pounds, giving an excess pressure of $\frac{1}{75}$ atmosphere. Weights are furnished in two sizes, in the shape of rectangular blocks of iron $4\frac{1}{2}$x2 inches surface, and about $\frac{1}{2}$ inch and 1 inch thick, respectively. The smaller ones are bored out to weigh $1\frac{1}{4}$ pounds each; the larger ones to weigh $2\frac{1}{2}$ pounds each.

Being placed on top of the air-chamber in successive pairs (one on

[1] New York Medical Journal, October 18, 1884; and other places.

[2] The shelves have been raised to avoid catching the weight of the escape-valve.

Fig. 2.



Sectional diagram of Improved Apparatus (drawn to scale).

The parts are numbered in order of attachment, and in direction of air-current, as shown by the arrows:—From window through compression bellows and gasometer to mask; from mask through rarefaction gasometer and bellows to discharge pipe, which may also be connected with window if desired.

each side, to preserve balance), they bring the pressure up to any desired amount, not exceeding $+\frac{1}{30}$ atmosphere. Thus:

Cylinder and bottom weights,	= 10 lbs.	= $+\frac{1}{75}$	atmosphere.
$2\frac{1}{2}$ lbs. (two small weights) additional	= $12\frac{1}{2}$ "	= $+\frac{1}{60}$	"
$2\frac{1}{2}$ " (two small weights) "	= 15 "	= $+\frac{1}{50}$	"
$3\frac{3}{4}$ " (one small weight and one large weight) "	= $18\frac{3}{4}$ "	= $+\frac{1}{40}$	"
$6\frac{1}{4}$ " (one small weight and two large weights) "	= 25 "	= $+\frac{1}{30}$	"

The air-cylinder is furnished with two goose-necks, one (8) for the attachment of the tube (6–7) from the bellows, conveying compressed air, the other (9) for attachment of the tube (10–11) connected with the stopcock (12) and face-mask (14) or mouthpiece, through which the patient inhales. A perforation two inches in diameter is fitted with a screw cap carrying a hook on which a sponge may be placed, which is to be saturated with any volatile medicament (*e. g.*, terebene) that may be desired. The cap likewise contains a smaller perforation, into which an air-gauge may be fitted. When the gauge is not in use this is closed with a rubber plug. Still another perforation in the top of the air-chamber is fitted with a valve, which permits escape of air should too much be sent over from the bellows. This valve, which is superior to my own arrangement for the same purpose, is the ingenious device of Mr. F. Metzger, of this city, who now makes the apparatus in every respect according to my instructions, and who has devoted much time and care to the details of construction in order to secure both strength and lightness. The escape-valve is composed of two flat plates of brass, the upper perforated, the lower unperforated. They are held in apposition by a spring, and when in apposition no air escapes. The lower plate carries a chain, thirty-five inches long, which is attached to a weight which rests upon the floor of the water chamber. Should too much air enter the cylinder, lifting it too high, the plates are pulled apart, the air escapes through the perforated plate, and the cylinder falls to the proper level. This obviates any liability to splashing of water, which before this attachment was made would occur if attention were not paid to a line painted on the cylinder to indicate cessation of pumping. By means of the automatic escape-

valve we are enabled to introduce a continuously acting pump if desired.

As an additional precaution against splashing, the air-chamber and overflow-tank are each provided with a deflecting hood about one and one-half inches wide, and inclined at an angle of 45°.

With these improvements over its original construction, being the gradual suggestions of six years' experience, the compressed air apparatus is felt to be complete. There is no particular in which I find it deficient, or can now think of a change which would increase its mechanical convenience or therapeutic efficiency.[1]

I have for some time been endeavoring to combine with it an apparatus for expiration into rarefied air; a procedure which has a certain limited degree of applicability, being sometimes employed alone, sometimes in combination with the inhalation of compressed air. There was no difficulty in constructing an apparatus merely for expiration into rarefied air. All that was necessary was to reverse the connection with the bellows, so that the latter would take air from the gasometer and deliver it into the room or the street; and to suspend the air-chamber from a small pulley, counterpoising it with weights varied according to the desired negative pressure. The mechanical arrangement of pulleys and weights, the devices for adjusting the weights, etc., are so obvious and so easily understood by looking at the instruments before us ("Rarefied Air," Fig. 2) that description in detail is unnecessary. Mr. Metzger has made the various parts with his accustomed care, and has used a wire rope in preference to one of hemp. The difficulty lay in the combination of the two gasometers into one instrument, by means of a bellows which should at the same stroke compress air for delivery into one cylinder and rarefy air in order to exhaust the other cylinder. As we could not get any bellows manufacturer to construct this, Mr. Metzger at last made an experimental one with his own hands (see Fig. 2), and it answers the purpose perfectly. It is in reality two bellows mounted back to back on the same frame. The downward stroke of the lever compresses the upper bellows and expands the lower one. The recoil of

[1] My friend, Dr. D. D. Stewart, suggests that care be taken that the rubber tubes employed are not of the variety weighted with lead.

the spring of the upper bellows expands that one and compresses the other. There is no communication between the two bellows. By means of a tube (2–3) passing out of a window-board (1), the external opening being protected by wire gauze, the supply of air for inhalation is drawn from out of doors.

When both instruments are used together, as I show now, being brought into communication through the lungs of a patient by means of a double stopcock connected with the face-mask or mouthpiece, the route for the air is as follows:

(*a*) From the street, (*b*) through the upper bellows, (*c*) to the compression gasometer, (*d*) thence to the lungs; (*e*) from the lungs (*f*) through the rarefaction gasometer (*g*) to the lower bellows (*h*), which expels it into the room or into the street.

Apparatus for warming, drying, or moistening the inhaled air may be interposed at any desired point between the window and the patient.

The compression and rarefaction are made in the respective bellows. The gasometers act, to a certain extent, as reservoirs, but chiefly as intermediate regulating chambers, rising and falling to maintain a constant pressure in exact accordance with the amount of weight placed upon them, and independent of the volume of air inhaled or exhaled. Their available capacity is about 800 cubic inches.

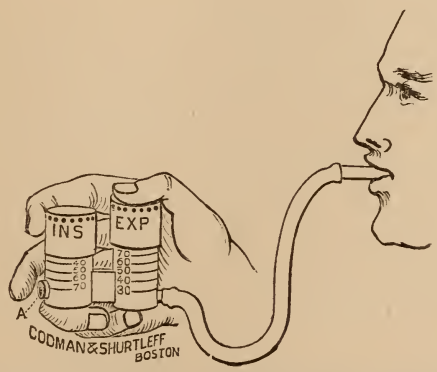
For purposes of observation sufficiently accurate for clinical comparisons, though not for physiological data, the air-chambers carry a scale of cubic inches, enabling us to see the approximate volume of air inhaled or exhaled at each respiration.

I have yet to describe the stopcock by which the ingress or egress of air to or from the face-mask or mouthpiece is governed. It is modelled on the cornet piston, and consists (Fig. 2)—(*a*) of a central barrel (*b*), carrying a hollow cylinder (*c*), controlled by a spring, and (*d*) a horizontal tube communicating freely with the central barrel. The face-mask or mouthpiece (14) is connected with the proximal extremity (13) of the horizontal tube; the delivery tube of the compression gasometer (10–11) with its distal extremity (12). The inner central cylinder contains two, or rather three, perforations. Two of these are opposite to each other, and when brought in line with the

horizontal tube, form with it a continuous channel. In this position air is allowed to pass from the compression apparatus through the mouthpiece or face-mask to the air-passages of the patient. The other perforation has no opposite fellow, and is on the side facing the mask or mouthpiece, so that when it is brought opposite the horizontal tube the air enters from the patient's lungs, and escapes through the inferior opening (15) in the central barrel. If it is desired to have exhalation made into rarefied air, this inferior extremity of the central barrel is connected with the rarefaction gasometer (16, 17, 18). If the rarefaction apparatus only is to be used, the attachments to the compression gasometer are, of course, omitted.

If it is desired to have the patient exhale into compressed air, instead of connecting the stopcock with the rarefaction gasometer, we can insert into this lower opening (15) an expiration resistance valve, as made for me by Messrs. Codman & Shurtleff, of Boston, and described in the *New York Medical Journal* of December 3, 1887.

Fig. 3.



Resistance Valves in use. By occluding the perforation, A, with the finger, either phase or both phases of respiration may be affected, as desired. When the finger is lifted, air passes freely beneath the valves, and no effect is produced.

If it is desired to have him inhale rarefied air, instead of connecting the stopcock with the compressed-air apparatus, we insert into the horizontal tube (12) an inspiration resistance valve.

The "resistance valves"—which I have called by that name because they offer a resistance to the passage of air—are simply

little cylinders, containing ebonite valves controlled by spiral springs. The tension of the spring is regulated by screwing the cap of the cylinder, and a scale engraved upon the side gives its value in fractions of an atmosphere. The expiration valve is arranged to move *toward* the perforated distal end of the cylinder, thus allowing the expiration current to escape whenever the pressure of the expired air reaches the indicated figure. The valve of the inspiration cylinder is arranged to move *from* the perforated top of the cylinder, thus allowing the inspiration current to pass toward the patient whenever the rarefaction within the lung reaches the indicated figure. The two valves suitably mounted in one instrument may be used independently of the gasometers (Fig. 3).

Thus, with these two gasometers, the double bellows, and a pair of resistance valves, we are able to obtain the mechanical satisfaction of all the conditions required for any of the following therapeutical expedients:

1. Inhalation of compressed air.
2. Inhalation of rarefied air.
3. Exhalation into compressed air.
4. Exhalation into rarefied air.
5. Inhalation of compressed air with exhalation into compressed air.
6. Inhalation of rarefied air with exhalation into rarefied air.
7. Inhalation of compressed air with exhalation into rarefied air.
8. Inhalation of rarefied air with exhalation into compressed air.

If a pump and motor be substituted for the foot-bellows, the only inconvenience connected with office use of the apparatus can be obviated. The additional cost is not excessive, and it leaves the apparatus still well within the price of any other capable of doing the same work.[1]

My endeavor has been, throughout, to use the simplest and least expensive methods and mechanical devices, and I have tried to

[1] Mr. Metzger sells either the compression or rarefaction apparatus singly, with all necessary attachments, for $30, and the double apparatus for $50. With pump and electric motor, the cost would be increased from $25 to $35 additional. Codman & Shurtleff sell the resistance valves for $5 a pair. Both manufacturers inform me that the time necessary to secure accurate adjustment of all parts of the apparatus, in order to insure uniformity of pressures, and the present limited demand for the instruments, render it impossible for them to reduce these figures still further, as I have requested.

induce the manufacturers to reduce their prices to the lowest point, in order to secure the widest possible and freest possible diffusion of the method, which I look upon as one of the most valuable within the reach of the therapeutist. The therapeutic expedients most generally employed are inhalation of compressed air and exhalation into rarefied air, and I shall speak of these only, and briefly.

In using the apparatus the patient should preferably stand, but he may sit, if necessary. If a face-mask is used, it should be accurately adjusted over nose and mouth, and made to lie close to the cheeks to prevent escape of air. A glass mouthpiece may be used instead of a face-mask in many cases, and it is unnecessary to close the nostrils, as nose and mouth cannot be conveniently employed together for forced respiration; and there will be no interference. Each patient should have his own mask or mouthpiece.

At first the smallest pressures only should be employed ($\pm \frac{1}{70}$ or $\pm \frac{1}{60}$ atmosphere), and these may be gradually increased according to indications. It is never advisable to exceed $\pm \frac{1}{30}$ atmosphere. From ten to fifty or even one hundred respirations, occupying from five to twenty minutes, are made continuously, beginning with the smallest number and gradually increasing. After inhaling for a while, say ten minutes, a rest of ten minutes or more is taken, and the process repeated. This may be gone through with once, twice, or three times daily, according to indications. Where the patient has an instrument at his home, it is employed more frequently than when it is necessary to visit the physician's office in order to obtain the facilities.

The physiological effects of these expedients are very apparent. The effect upon the circulation, of increasing the pressure within the thorax as compared with the pressure upon the periphery, by inhalation of compressed air is, in brief, to drive the blood toward the point of least pressure—that is, out of the thorax. On the other hand, the effect of exhaling into rarefied air, thus increasing the pressure upon the periphery as compared with that within the thorax, is to drive the blood into the heart and lungs. This effect, however, for obvious reasons, is much less in degree than when rarefied air is inhaled.

The effect upon the respiratory act is equally obvious. If the pressure within the thorax be made greater than that without, ex-

pansion will be effected with less muscular effort. The air-cells are fully dilated, and occluded areas of lung tissue are forced open, where the pathological process is not of such a nature as to prevent this result. There is also pressure upon the inflammatory congeries of new cells and upon congested bloodvessels, tending still further to increase the amount of available lung tissue, and to a certain extent to relieve congestion and promote resolution of inflammation. It acts as pressure by means of the bandage acts in external inflammations. The activity of the pulmonary circulation being increased, as well as the activity of the systemic circulation, a greater volume of blood passes through the pulmonary capillaries in a given time, and is exposed to a greater amount of oxygen under pressure; conditions which slightly favor the absorption of oxygen. As the increased volume of blood carries a greater number of corpuscles, theoretically it would seem that there should be a greater activity of oxygenation, and this has been confirmed by exact investigations conducted by Waldenburg, Speck and others, of which Oertel gives a full summary in his admirable treatise. Subsequent expirations are freer and longer, and a greater amount of carbonic acid is given out, thus increasing pulmonary ventilation. Masses of desiccated secretion, desquamated epithelium, etc., are mechanically dislodged, and their expulsion by cough facilitated; so that, at first, cough is increased. It is afterward lessened by correction of the conditions producing it. In the light of our present knowledge of auto-intoxications, the cleansing of the lungs from these decomposing masses of effete material must be considered of no little advantage.

Expiration into rarefied air facilitates expulsion of the air within the alveoli, and, consequently, contraction of the chest is more complete. Subsequent inspirations are easier and deeper.

By exhaling into rarefied air, after inhaling compressed air, we emphasize the difference between the two processes, and make an alternation of pressure-effects upon the vessels. All the effects before noted are thus heightened. An improved respiratory habit—that is to say, a fuller, freer, and more regular, habitual respiration—is brought about, and the gain in vital capacity is very great. Not only is the exhalation of carbonic acid favored, but the subsequent absorption of oxygen is augmented, on account of the greater extent

to which emptying of the lungs has been secured. The alternating pressure upon the alveoli increases the elasticity of the lung tissue. The alternate filling and emptying of the vessels stimulate circulation, increasing both rapidity and volume.

I am aware that doubts have been cast on the possibility of increasing the absorption of oxygen. Be this as it may, the effect clinically is vastly to stimulate and increase the nutrition of the patient. By careful observation I have been led to attribute this increase of nutrition to the ease with which the patient is able to dispose of the greater amount of nutriment which we administer. A patient over-fed, but not taking inhalations of compressed air, will not increase so rapidly in weight, as one who is over-fed and at the same time taking these inhalations. A patient taking inhalations of compressed air and not over-fed, will not increase in weight. I have thought that the great gain was due to the stimulation afforded to the secondary assimilation and to the general metabolism. Tissue-respiration and the activity of the chylopoietic processes are increased by the increased activity of circulation, the heightened blood-pressure, the increased pulmonary gaseous exchange, and also by the pressure transmitted from the diaphragm to the abdominal organs. Patients with pulmonary troubles who are taking iron will take larger quantities and bear it better. This is also the case in anæmia and chlorosis. One of the earliest manifestations of improvement is promotion of sleep, due partly to relief of cough and improvement of respiration, partly perhaps, to improvement of metabolic processes and consequent elaboration of physiological hypnotics. There is also an obvious gain afforded by the exercise to the chest muscles, to which a portion of the augmentation in vital capacity must be attributed. Some four years ago I published a number of personal observations on the increase in the chest capacity from inhalation of compressed air.[1] The results were not so remarkable as some of those published by European observers, but they indicated a gain of from twenty to thirty cubic inches in respiratory capacity and from half an inch to one and a half inches in chest expansion. This was in patients in

[1] On the Value of a Proper Respiratory Diet in Phthisis. The Medical News, August 2, 1885.

whom chest expansion and respiratory activity had been impaired by condensation of the lung from phthisical or pneumonitic processes or from the presence of pleuritic effusion. Pressure stimulates absorption of pleural effusions, and after the effusion has been absorbed the inhalation of compressed air tends to hasten the re-establishment of the normal action of the lung.

In two cases of asthma I have overcome the spasmodic dyspnœa of the acute attacks by the inhalation of compressed air at rather great pressure ($+\frac{1}{30}$ to $+\frac{1}{20}$ atm.). I suppose that the rationale of the effect is in part the paralyzing of the spasmodically contracted muscles of the bronchioles by the increased pressure of the air, and in part the relief of fluxionary congestion, by direct pressure upon engorged vessels; acting somewhat upon the principle of Monell's method of fixing the feet against the footboard of the bed, taking a deep inspiration and holding it as long as possible.

The pneumatic expedient which has afforded the best hope of permanent relief in asthma, especially when dependent upon emphysema, is, I think, expiration into rarefied air. This promotes collapse of the distended air vesicles and tends to relieve the lungs of the accumulation within them; and to a certain extent obviates the paralytic distention consequent upon the presence of a greater amount of air than they should normally contain. It cannot of course restore destroyed septa.

I have also thought, in common with many observers of large experience in pneumo-therapy, that the inhalation of compressed air had some good effect in the treatment of dilated heart, from the pressure exerted upon the pulmonary vessels and cardiac chambers. Latterly I have employed in this affection expiration against pressure by means of the resistance valve, a still more physiological expedient. I have not used expiration into rarefied air in these cases, believing it to be counter-indicated, although it has been recommended by some.

There are many other obvious clinical applications of this method of treatment, but my object in bringing the subject before the College has been more particularly to show what can be done mechanically by a simple apparatus which may be placed in the patient's home, and not to refer especially to therapeutic uses. These would

depend upon the views of the physician, and the experience which he obtains in the management of the method. The greater that experience the more confidently will he extend it.

The effects are purely physical and mechanical, and easily deducible from a knowledge of the physical and mechanical effects upon normal circulation and respiration.

The one great advantage of the compressed-air machine is that it will enable us to treat *at home*, patients with phthisis or threatened phthisis, and especially to prevent the development of tuberculosis in patients having—with, perhaps, an apical catarrh as well—insufficient expansion, insufficient aëration of blood, insufficient nutrition, sluggishness of circulation, and impairment of the muscular apparatus of the thorax. These patients are often sent chasing around the world in the vain search for a restorative climate, but very few receive benefit, except, of course, such good as may be incidental to an outdoor life. There are, however, a very great majority of persons whose circumstances preclude the possibility of leaving home. For these, pneumatic treatment is all-important. I have elsewhere indicated how we may by a few simple expedients imitate many of the features of special climates.[1]

I have not had a wonderful number of cases under my care, so that I cannot give any extended statistics; but every year I see several cases of phthisis benefited, and three or four cases improved in a remarkable manner. There is one case in particular which is in my mind, because I received a letter from the patient to-day. It is that of a Bostonian, thirty-two years of age, who spent last winter in Philadelphia, and although the weather was unfavorable, and she had an intercurrent pleurisy, made a good recovery. She had a large cavity in the right lung and a smaller cavity in the left lung. All of her father's family, except her father, and many of her mother's family, died of consumption. Her mother was said to have had consumption, but recovered under open-air life, forty years ago, and is hale and hearty. The father, who is some sixty-five years old, has fibroid phthisis, and is now breaking down. She was seen by Professor DaCosta before the beginning of the treatment and at the end,

[1] Artificial Climatic Effects for "Stay-at-Homes." Philadelphia Medical Times, February 6, 1886.

and he was able to verify both the pulmonary conditions and the great gain. When she began treatment her weight was 107 pounds; when she left Philadelphia in May it was 135 pounds, and in a letter received to-day, she stated that she now weighs 152 pounds. She had an evening temperature of 100.5° F., and all the troublesome symptoms of phthisis in the stage of softening. She went home with a normal temperature and troubled only by more or less coughing, which has steadily decreased. The treatment was simply that of nutrition; *i. e.*, over-feeding with nitrogenous and fatty aliments, whiskey and hot water, aided by inhalation of compressed air, which enabled the increased quantities of food to be disposed of. The patient was kept out-of-doors as much as possible, and instructed to take deep and slow respirations continuously. Medication was principally symptomatic, for relief of cough, for disinfection of the alimentary tract, or for stimulation of digestion. She was given, at different times, creasote, iodoform, arsenic, amyl nitrite, and compound spirits of ether, with inhalations of terebene, chloroform, or ethyl iodide from the Yeo (perforated zinc) respirator, or from the Oliver nebulizer. I must not forget to add, in testimony to the value of a method of treatment now abused in neglect as formerly in extravagant laudation, that the Bergeon injections of hydrogen sulphide and carbon dioxide served a useful temporary purpose in controlling recurrent suppuration in the partially healed cavity of the right lung, following the pleurisy.

This woman evidently inherits a consumptive diathesis, but also a good recuperative force, which only required to be put under favorable conditions. She had a machine at her hotel, and she obeyed orders as to diet with religious exactitude.

Patients who recover like this, stay recovered. Among my own patients are the two who were first treated with my apparatus in 1883, and reported upon in 1885, who are still well; and some of my brother's cases, referred to at the opening of these remarks, have been well for twelve and thirteen years. Even cases which do not recover can be greatly improved. Life may be not only prolonged, but made much more comfortable. And to return to what I deem the most important consideration, the treatment is essentially a home treatment, and it is within reach of those of moderate means.

THE TREATMENT OF FRACTURES OF THE CLAVICLE.

By G. G. DAVIS, M.D., M.R.C.S. Eng.

[Read November 6, 1889.]

The many forms of bandages and apparatus which have been devised for the treatment of fractured clavicle simply bear witness to the fact that our means of treating this troublesome accident are not as yet perfect, and anything that will tend to elucidate the subject and thus lead to better results is worthy of consideration. Some time ago, having had a case of posterior dislocation of the acromial end of the clavicle, I was impressed with the advantages of not only effecting a reduction in the recumbent position, but of applying the retaining dressing at the same time. The success which followed the procedure in that case (reported in the *Annals of Surgery*, May, 1887), induced me to apply the same method to fractures of the clavicle. The ease with which most of the deformity which is present in fracture of this bone disappears when the patient lies flat on his back, is well known. The object aimed at was to apply a dressing that would retain the parts in as near the position which they occupied when the patient was on his back as possible. To effect this a plaster-of-Paris dressing was used. The procedure at first adopted was to make the patient lie flat on his back and place into the hollow above the clavicle, above and to the inner side of the seat of the fracture, a small hard roll of patent lint. This was kept in place by a couple of adhesive straps crossed over the shoulder. The whole shoulder was then covered with a double layer of lint, over which several layers of gauze, well impregnated with plaster-of-Paris cream, were placed. A pad was next placed in the axilla, and the whole firmly bandaged with a Desault bandage. The patient was kept on his back until the plaster had firmly set.

My object in this dressing was to make a mould for the affected shoulder in which the clavicle lay in a groove supported in its entire length.

The plaster having hardened while the patient was recumbent, a mould was formed of the parts while the fracture was in the best possible position, and it retained this form even when the patient was allowed to rise.

For a short time after the application of the dressing things went on well, but I soon found that the muslin bandages used failed to insure sufficient immobility of the parts, and, consequently, the shoulder dropped, and the common angular deformity showed itself. When, after several trials, I found this to be the case, I modified the dressing by adding plaster-of-Paris bandages to the muslin ones, and the results immediately improved. The dressing eventually adopted was applied as follows: The patient being placed on his back on a hard level surface, with no pillow for the head, as good a reduction of the deformity as possible was made. A small hard roll of lint was then placed above the outer extremity of the inner fragment immediately above the seat of fracture, but not pressing on the pointed extremities of the bone. A pad large enough to keep the arm well away from the side, but not hard enough nor of sufficient size to act as a fulcrum and pry the shoulder outward, was placed in the axilla. The shoulder, elbow, and opposite healthy axilla were then carefully covered with a layer of cotton. The patient being partially raised by an assistant, the arm of the affected side was bound to the body with a muslin bandage, the turns beginning above and descending to the elbow. With another bandage the third roller of Desault was applied, the initial extremity being placed in the sound axilla and the roller carried across the back, over the affected shoulder, down in front of the arm, beneath the elbow, and back to the healthy axilla. Crossing at this point the initial extremity, the roller was carried across the front of the chest over the affected shoulder, down behind the arm beneath the elbow, and back to the healthy axilla. These anterior and posterior turns were then repeated with a plaster bandage, and the dressing finished by a series of circular turns around the body. The hand was placed in a sling hanging from the neck, and the patient kept quiet on his back

until the plaster had hardened. He was instructed to return at once if the dressing was uncomfortable, but otherwise to present himself in a week. On his return the dressing was examined, and if the plaster bandage with its edges had irritated any part, additional cotton was tucked in between it and the body. He was then dismissed for another week. On his return, the bandage having been on continuously for two weeks, it is desirable to inspect the parts and renew the cotton beneath. With a saw and scissors the plaster bandage is opened on the side beneath the healthy axilla and the whole dressing removed, the patient being on his back. The surface is then cleaned with soap liniment and any irritated parts dusted with powdered oxide of zinc or covered with zinc ointment spread on lint, the small pad over the clavicle is inspected and clean cotton reapplied. The original plaster dressing is then replaced, and while being held shut by an assistant is confined by turns of a plaster bandage passed circularly around the body. This second dressing may be allowed to remain on an additional two weeks, after which a handkerchief dressing, such as that of Mayor or Gosselin, may be used if desired. If, at the end of the first two weeks, the plaster dressing is much broken, it may be desirable to apply a new one.

This dressing is not intended as a universal bandage for all fractures of the clavicle, but rather for those obstinate cases which occur in youths sometimes, and usually in adults, in which the fracture is complete and the tendency to angular deformity from dropping of the shoulder marked. Its object is to raise and keep the lowered external fragment up to and level with the internal one, and it does this better than any other dressing I have tried. One is led to believe, from the good results claimed by confining the patient to bed, that in order to reduce the deformity completely, it is only necessary to place the patient flat on his back. This may be the experience of some, but it certainly is not mine. Time and time again have I placed patients in the recumbent position and attempted a complete removal of the deformity, but it is comparatively rarely that I have been successful. The angular deformity can sometimes by this means be completely or almost completely reduced, but if the fracture is a bad one, there will almost always be, in spite of anything

that can be done, a slight overlapping of the fragments. The point of the shoulder is kept out and away from the body by the clavicle, and when this is broken the muscles connecting the upper extremity with the trunk and the tissues which surround and are attached to the clavicle, particularly the subclavius muscle, all tend to draw the outer fragment inward, and I have yet to discover some means of obviating this. The attempt of Desault to do this by placing a pad in the axilla and prying the shoulder out, is almost universally regarded as failing to accomplish the object, and for that reason in my dressing it is not attempted, but a pad of such a size is used as will, first of all, prevent the arm and chest from coming into contact and thus becoming irritated; and, secondly, as will prevent the arm from being drawn still further inward than it is when the patient is flat on his back.

The pad over the inner fragment must be nicely adapted and not too large. It should be large enough than little more to fill the normal hollow which exists at this point.

The cotton should be spread over every part that is liable to become irritated by friction of the bandage, so that, usually, I envelop the chest in a thin broad layer in front and behind, with a thicker layer beneath the healthy axilla and the elbow and forearm of the affected side. This must be so carefully applied that the surgeon will feel perfectly secure in leaving the dressing on two weeks without the expectation of finding any serious excoriations on its removal. If so desired, a pad of cotton may be placed on the sound shoulder, and a few additional turns made over it and beneath the elbow of the affected side.

I have been trying this mode of treatment for the past two years, and, as I see about twenty cases of fractured clavicles a year, have had a fair opportunity of testing it. The conclusion I have come to in regard to it is, that in those marked cases with a tendency to angular deformity I can obtain better results with it than with other dressings.

I do not intend to claim that by its means many cases can be cured without deformity, but to present it as an effective means of obviating the tendency to angular deformity. In only one of my adult cases would I be willing to state that the patient recovered without any

deformity. In this case the angular deformity was marked, but the fracture being more transverse than usual, there was little or no overlapping. In another instance, that of a thin, angular young man with an oblique fracture of the middle of the bone, consolidation took place without any angular deformity whatever, but on the back of the bone a small sharp angle showed where there had been some overlapping of the fragments. This case was a crucial test of the method because, on account of the thinness of the subject, any deformity could at once be detected, and the fracture was one of the worst kind, being complete with overlapping and very marked deformity. In neither this nor many other of my cases, when the fracture was a marked one and oblique, could I by any device reduce the deformity absolutely—there would be small projections either on the front or back of the bone which showed that some slight amount of overlapping still persisted.

In my last case, a young girl, treated by this method, the last part of the treatment was conducted by Dr. Henry Deaver, and he informed me that she recovered without any angular deformity whatever, but that there was some shortening of the injured bone as compared with the opposite one. This was undoubtedly due to over-lapping, proof positive of a complete fracture.

In cases of children and others, in which from the situation or character of the injury the tendency to angular deformity is not marked, of course other methods of treatment will yield satisfactory results. The use of plaster-of-Paris dressings in the treatment of these cases is not presented as anything new, but it is the principle of effecting as complete reduction as possible while the patient is on his back, and the application of the dressing while in this position, that is submitted for your consideration.

DISCUSSION.

Dr. John Ashhurst, Jr.: I have listened with a great deal of interest to this paper, which certainly presents a mode of treatment of fracture of the clavicle which seems to be eminently successful. There is not a great deal to be said in regard to fractures of this bone; but I am glad to call attention, as I have done before, to the indebtedness of surgeons to our late Fellow, Dr. Edward Hartshorne, who wrote one of the best papers on fracture of the clavicle that has ever been laid before the profession.

It has always seemed to me that the essence of the successful treatment of fractures of the clavicle, as well as of other bones, must depend upon a consideration of the causes of deformity. I think that the reader of the paper did not bring out the exact causes of deformity quite as clearly as he might have done. I am in the habit of illustrating this subject to my classes by reference to the anatomy of the lower animals as compared with that of the human being. The germ of this idea may be found in the writings of the late Mr. Callaway, and it is also worked out in Dr. Hartshorne's paper. What has been called the shoulder-girdle consists of a system of bones, embracing the sternum, the two clavicles, and the scapulæ. In the lower animals—for instance, in the domestic fowl—the two clavicles and the breast-bone are represented by one bone, the pull-bone. In the animals that use their fore-limbs for purposes of progression, we find that the arrangement is such that the anterior extremities project from the trunk in an anterior direction, the object being that the weight may be sustained upon the fore-legs. In the human being the anterior extremities are used for prehension, and here the clavicles are introduced to throw the attachment of the limbs to the sides of the body, thus giving a wide range of movement to the arms.

It is easy to understand that when the continuity of this shoulder-girdle is interrupted, as by fracture or dislocation of the clavicle, the natural support of the part is lost, and the powerful muscles attached to the scapula will draw the arm forward; and in fracture of the clavicle the actual cause of the deformity is not so much the dropping of the shoulder—although I am not prepared to deny that the weight of the arm has some influence—but the action of the muscles attached to the scapula rotating this bone forward to the side of the chest. Any one who will try the experiment, in such a case, of bringing the scapula back into place, will find that the clavicle is also brought into position. Hence, the most important point in the treatment of a fractured clavicle is to fix the scapula; if that can be done, the fragments of the clavicle will fall into place.

Fixation of the scapula can be accomplished, as Dr. Davis mentioned, by confining the patient to bed on a hard mattress. I once succeeded in curing a fractured clavicle in this way without any deformity. The patient was a

young woman, who was induced to lie in bed perfectly quiet for three weeks, by the statement that this would prevent the formation of a lump in the neck. In the large majority of patients, however, this is not necessary nor even desirable: a laboring man would rather have a slight deformity than to lie on his back for three weeks. We are, therefore, obliged to resort to some other mode of treatment. The essential point is to fix the scapula; if that can be done, almost everything else can be neglected.

There is, however, another cause of deformity, pointed out by a French surgeon, M. Anger. This is the resiliency and elasticity of parts attached to the inner fragment, the sterno-cleido-mastoid muscle and the ligaments, tending to draw the inner fragment upward. This is not due to contraction of the muscle, but to the natural resiliency of the parts, the weight of the arm, which holds the clavicle down, being removed. This may be counteracted, as mentioned by Dr. Davis, by the application of a small compress over the inner fragment. This may be held in place by a splint of plaster, gutta-percha, or moulded leather, or by strips of adhesive plaster, the latter form of dressing being that which I generally adopt.

Having fixed the scapula and the inner fragment of the clavicle, all that remains to be done is to put the arm into an easy position, which will prevent muscular contraction. For this purpose I prefer the Velpeau position to any other. I believe that it relaxes the parts better and allows the bone to fall into place more satisfactorily than any other position. Some of the most popular modes of treatment have the opposite end in view—that is to put the muscles on the stretch. Such are the methods of Dr. Moore, of Rochester, and Dr. Sayre, of New York. The aim is to put the pectoralis major on the stretch, the theory being that this will pull the fragments into place. This does not seem to me to be a desirable mode of accomplishing the object, and I do not believe that the muscle remains tense when the dressing is completed, but that this really accomplishes its object by insuring a fixed position of the scapula and arm.

The essential points of treatment, then, are, fixation of the scapula, fixation of the inner fragment of the clavicle, and the placing of the muscles at rest. If these objects can be carried out, the results are quite satisfactory; but I doubt whether an absolutely perfect cure is ever obtained, at least in adults. A certain amount of shortening is, I believe, inevitable.

Dr. J. William White: I happen to have under my care at the University Hospital a patient who has the rare accident of fracture of both clavicles, the first case that has come under my notice. The accident is extremely rare. Stimson was able to collect only twenty-eight reported cases. Among 690 cases of fractured clavicle treated at the Pennsylvania Hospital not one was bilateral. This case is also of interest in connection with the treatment. All that has been said in regard to the difficulty of fixing the scapula is more than doubly intensified by the existence of double fracture. This man has been treated by the recumbent position alone, with

the head slightly raised and the body held in position by heavy sand bags—one on either side at the tip of the Acromion process. He has been under treatment for two weeks and five days. The result has been good union with a little deformity on both sides. As the accident is such a rare one, I have taken the liberty of having the patient brought here for the inspection of the Fellows of the College. When the patient is treated in the recumbent position the arms should, theoretically, be placed in the Velpeau position and kept there. This, of course, was hardly possible in this case; but, in spite of it, the result on each side is quite as good as the average after fracture of the clavicle.

Malgaigne saw only one case of this kind, and of the six that he was able to collect, there was non-union on both sides in three. In eleven of the twenty-eight cases collected by Stimson the cause of the double fracture was a compressing force acting in the transverse diameter of the body on both shoulders, as the caving in of an embankment or the fall of a heavy weight. In one case the fracture was on both sides due to direct force from the kick of a horse, a hoof striking each clavicle. In other cases the first clavicle has been broken by direct force and the second by indirect force from the fall to the ground. In this case the man is unable to tell how the injury was received. All that he remembers is that he was on the platform of a car, and when he next recollected anything, he was on the ground by the side of the railroad. It is probable that both fractures were the result of indirect violence, as no marked contusions were found. In Malgaigne's case at the termination of treatment there were marked deformity, narrowing of the upper portion of the thorax, projection of the scapula, etc. The shoulders could be approximated anteriorly until there was a space of but a few inches between their tips.

Dr. O. H. Allis: There is very little to be added to what has been already said. It strikes me that the point brought out by Dr. Davis is an exceedingly important one, viz.: that of placing the dressing on the patient *while he is in the recumbent posture.* Every surgeon knows that when a dressing is applied in fractured clavicle to a patient in the erect posture, it is displaced when he lies down, or that if applied while the patient is in the recumbent position, it is displaced when he rises. The placing of the patient in a plaster-of-Paris dressing while in bed, and keeping him there for the first week or two, seems to be a valuable point. Dr. Levis once said that he saw no reason why a patient should not go to bed for fractured clavicle as well as for fractured femur. If patients were compelled to go to bed for two or three weeks, there would be less said in regard to deformity.

In the only case in which I used a dressing similar to that suggested by Dr. Davis, I had the patient stand in front of me and applied a plaster dressing to the arm, shoulder, and chest. I then adjusted the fracture and applied a compress of plaster-of-Paris directly over it. In this case I secured by a single dressing an excellent result. As far as my experience goes, I look

upon this suggestion of Dr. Davis as representing an advance in the treatment of this injury, and one that merits universal trial at the hands of the profession at large.

Dr. G. G. Davis: In preparing this paper I had one object in view, and that was to present what I thought was, at least to a certain extent, new; and I therefore did not consider the pathology and many other points which have been discussed so much. According to my experience, the deformity is of three kinds. In the first place, the inner fragment tilts up, whether from contraction of the muscle or from elasticity of the parts need not be considered. In the second place, I believe that the outer fragment does fall down a little. In the third place, the outer fragment falls inward. To keep the inner fragment down, I place a compress over it. The reason why the outer fragment approaches the chest, is that the support of the clavicle is gone and the scapula rotates forward. When the patient is placed flat on his back the scapula revolves backward and the outer fragment goes outward. Then, by raising the elbow very slightly, you can get, as near as it is possible, a perfect adjustment of the fragments.

I should have liked to hear more remarks, such as those of Dr. Allis, showing that trial had been made of the immobile apparatus. Velpeau himself recommended a fixed dressing to be applied over his ordinary bandage. My idea is to apply the dressing with the patient on his back and allow it to set. The patient will remain quiet for half an hour, when he would not do so for two weeks. I first tried to keep the fragments in place by moulds over the shoulder. This answered for a few days; but then the dressings would slip and the angular deformity would recur. If a trial be given this method, it will not be long before an idea as to its value can be formed.

INSANITY AFTER ACUTE SURGICAL OR MEDICAL AFFECTIONS.

By H. C. WOOD, M.D., LL.D.,

PROFESSOR OF THERAPEUTICS, AND CLINICAL PROFESSOR OF NERVOUS DISEASES, UNIVERSITY OF PENNSYLVANIA.

[Read November 6, 1889.]

In response to the request of the Secretary of the College of Physicians for an article to be read at the present meeting, I have ventured to call the attention of the Fellows to the subject of "Insanity following Acute Disease." I am aware that most alienists are more or less familiar with this form of insanity, and have generally correct ideas concerning it, but my own experience has shown me that general practitioners of medicine fail very often to comprehend the situation of the patient. Almost invariably the cases fall into the hands of the family doctor, so that it is very important that correct ideas be disseminated amongst the rank and file of the profession. There are, moreover, certain points which will still bear discussion even among alienists.

There is a tendency in writers on insanity to recognize as distinct diseases various varieties of mental disorder, which, according to my thinking, should be viewed simply as symptom groups. The evidences of mental disorder may vary when the brain lesion is apparently the same, so that an individual case may appear to belong to now this and now that special insanity. Congestion, even active congestion, may go hand in hand with exhaustion: failing nutrition, indeed, even pre-

disposes to local affluxes of blood by producing weakness of the vessels; and, according as now this and now that region of the brain is invaded by these local changes in circulation, so will the symptoms shift from day to day. The fact that two cases for a time present similar manifestations is no proof that they are essentially the same in their underlying cerebral condition, and the circumstance that two cases offer diverse symptoms is no proof that they are essentially unlike. I believe, although insanity following acute disease varies greatly in its symptomatology, that in almost all of the cases there is one common fundamental brain condition, and that this fundamental brain condition bears no specific relation to the disease which has produced it, but may be the outcome of an altered nutrition produced by an exanthematous disease, like typhoid fever, or by a diathetic disorder, like rheumatism; by a general blood-poisoning, like erysipelas; by an accidental traumatism of other portions of the body than the head, or by a surgical operation.

Readers of Krafft Ebing know that he describes, under the name of "Stupidity, or Primary Curable Dementia," a condition of cerebral exhaustion in which there is almost complete paralysis of the mental functions, with loss of nerve tone in every portion of the body, so that the patient remains in a condition of more or less profound stupor or stupidity, with shifting, kaleidoscopic, anomalies of motor and vaso-motor innervation, and at times also gives evidences of delirium, or of hallucinations. This primary dementia may be produced by starvation, by profound emotional shock, and by diseases of the basal bloodvessels. If old age, syphilis, or gout has produced excessive degeneration in the cerebral bloodvessels, or if an emotional shock has been so severe as to alter permanently the nutrition of the cerebral nerve cells, this so-called "Curable Dementia" may be an incurable affection. I can see, however, no more reason for separating such incurable dementia from the cases in which the lesions in the brain cortex are not so profound and permanent, than there is in nosologically distinguishing between cases of typhoid fever, whose malignancy is

so great as necessarily to end in death, and the so-called "walking typhoid."

Krafft Ebing also recognizes, under the name of "*Wahnsinn*," an affection which has been known to English and American writers as "delusional stupor," "*mania hallucinatoria*," "confusional insanity," etc., in which there is active delirium associated with an extraordinary abundance of hallucinations present in every sense-region, and with a great weakness of the whole nervous system, as shown by pronounced loss of mental power, amounting, it may be, to imbecility, by a tendency to stupor, by lack of muscular power, and by failure of nerve tone in every portion of the organism.

Krafft Ebing, himself, states that the two affections, "*wahnsinn*" and "primary curable dementia," clinically grade into one another, and that the underlying brain condition is similar in each affection. I myself firmly believe that these two so-called diseases are merely diverse manifestations of one and the same pathological condition, and that they should be considered as one disease. For this disorder, CONFUSIONAL INSANITY is probably as good a name as can be found, because it is already familiar to many, has no pathological import, and expresses a symptom which is not only common to all forms of the disease, but is a necessary outcome of the pathological state.

In various chronic diseases attended with great bodily and mental exhaustion, the brain tissue gradually passes into a condition of perverted and exhausted nutrition similar to that of confusional insanity. Probably most of us have seen in long-drawn-out cases of consumption such gradual impairment of the mental faculties, associated with a super-activity of the imagination, as renders the unfortunate individual a distress to himself and to his friends. As the disease progresses the mental disorder goes further, and, especially during the night, the patient becomes delirious.

Almost every history of shipwreck, followed by long exposure and starvation, affords a recital of failing mental power, accompanied by increasing activity of the imagination, until desire

and thought-pictures give rise to hallucinations, which are at first recognized by the sufferer to be false, but at last become to him living realities, luring him to leap overboard into what seems to him a land of plenty, but is, in truth, a watery abyss of death.

We must consider that the underlying nerve condition in each of these cases is one of a peculiar exhaustion, and it would appear that almost any form of acute exhausting disease may be followed by a similar mental state. I reported to this College some years ago a case of so-called acute, gouty insanity, which certainly represented the stupor form of confusional insanity; and gouty and rheumatic insanities are probably almost always representatives of this disease. Confusional insanity follows typhoid fever not very infrequently, and constitutes the bulk, if not all, of the cases commonly named puerperal mania. To it, also, belongs the so-called surgical insanity. Within the last year I have seen it developed after ovariotomy, perineorrhaphy, and after the removal of the breast for cancer. It may be, also, due to emotional strain, especially when the latter is sudden or accompanied by exhausting circumstances. Thus, I have seen it caused in an overworked, nervous bank cashier by the chagrin and worry due to his having cashed a large forged check, and in one of my cases it was the result of religious excitement and worry concerning the future of a dying man, superadded to loss of rest through nursing.

The reasons for believing that attacks of insanity following acute disease are identical in their nature, are both etiological and symptomatical.—*Etiological.* If we believe that an insanity has a specific relation to the poison of the disease which it has followed, we must consider that there are at least a half dozen specific insanities connected with acute diseases—a very improbable supposition: the symptoms develop at a time when the specific action of the poison upon the nervous organism has exhausted itself—namely, during convalescence: these insanities develop after diseases or affections in which there is no

known specific poison, such as childbirth, traumatism, surgical injuries, etc. Fevers, traumatisms, etc., which are followed by insane outbreaks, have one influence in common—*i. e.*, they all tend to exhaust or impair the nutrition of the nerve centres; and it is known that impairment of the nutrition of the cerebral centres by lack of food, combined with anxiety, is capable of causing symptoms similar to those which are present in insanities developed after disease.—*Symptomatical.* Though the cases vary very much in their details, the general scope of the symptoms and the general course of the disorder are identical. There is always mental confusion, a mixture of excitement and failure of mental power: further, the course and results of all these affections are identical, they nearly always ending in complete recovery if the case be free from organic disease.

In the mildest cases of mental disorder, after acute exhausting disease, the only symptoms may be those of enfeeblement of the general mental powers. It is remarkable how slowly, in some of these instances, the original mental constitution is recovered; indeed, I have sometimes thought that recovery is slower in milder than in severe cases. I have seen in a college student, in whom during the worst of the mental state following a bad typhoid the only symptoms were failure of memory and an inability to reason abstrusely, two years required for the restoration of the power of acquiring knowledge and doing high intellectual work. This mental enfeeblement may be associated with depression of spirits, but the depression of spirits is not so intense or so overpowering as in melancholia. A little observation will show that the emotional disturbance is not the dominant element in the case. When confusional insanity is fully developed, there is almost invariably a general lack of nerve tone, as shown by a feeble circulation and coldness of the extremities, by general muscular relaxation, and by failure of the digestive power. The tongue is often so heavily coated, and the breath so foul, that the inexperienced may be led to think that digestive disorder is the basis of the attack.

Little or no attention has been paid by writers to the bodily temperature, and, unfortunately, I have seen the disease almost exclusively in consultation, under circumstances which too often have rendered temperature sheets unattainable. It is certain, however, that the temperature varies in different cases. It may be normal, but in severe cases there is usually either an habitually low temperature, or a marked tendency to paroxysms of subnormal temperature. On the other hand, there may be a very distinct febrile reaction. This I have especially seen in puerperal cases. When fever exists at all, the swing of the thermometrical mercury is remarkable for its irregularity and its extent, and a very high temperature is often followed by a sudden and marked fall below the normal.

The mental symptoms may seem to be contradictory, since many of them are those which are commonly believed to be the outcome of paralysis of cerebral functions, and others are such as are sometimes thought to be evidence of excited, though perverted, cerebral activity. In the first group belongs that depression of consciousness which in its mildest forms may be shown only by a peculiar quietude and by apathy, but which in varying degrees of greater severity manifests itself by stupor, ever growing as the disease becomes more severe in intensity, until it deepens into a complete, persistent loss of consciousness. Another outcome of cerebral weakness is the peculiar mental confusion which is the most characteristic manifestation of the disease. It may reveal itself chiefly in the inability of the patient to talk coherently and persistently, words dropping out of the sentence, or being uttered imperfectly, because the mind is unable to get the right word, ideas changing in the middle of a sentence because the power of confining the attention to one consecutive line of thought is lost, so that the attempt at conversation on the part of the patient results in a jumble of half-sentences, clauses and words hopelessly intermixed one with another. Even, however, in mild cases of disease the mental confusion usually manifests itself, not merely in the inability of the patient to hold a connected conversation, but in his want of power to appreciate

persons and things about him. In the more extreme instances no objects or faces are recognized, and even in the very mild forms of the disorder the patient may recognize some of his friends, yet be unable to place himself, insisting that he is away from home, and pathetically begging to be taken to his own house. Clinical experience has shown that during the acute stage of a fever an inability to recognize familiar objects, and a confused, repeated request to be taken home, are of the most serious prognostic import; they are, however, in the disease now under discussion, of no such moment.

The confusion of the patient is not altogether the outcome of pure mental weakness, but is usually in part due to the extraordinarily numerous and vivid hallucinations which affect all the senses, and compete for recognition by the consciousness with impulses which really originate in external objects. The weakened judgment is unable to distinguish between the contending claims of subjective and objective sensations, so that realities and imaginations are intermingled into a hopeless chaos; moreover, the memory is pronouncedly affected, and old forms and familiar sights are forgotten, and the connection of external objects with the past is, for the time being, severed.

The delirium is commonly mild, and lacking in aggressiveness, but it may take on a very active form; or, the patient may be habitually quiet, but subject to paroxysms of fury resembling those of acute mania. In these outbreaks the patient may be violently erotic; indeed, any form of mania may be counterfeited. More commonly, however, underlying even the aggressiveness and violence there is a foundation of fear which often resembles that of delirium tremens, and when with this condition of fear there is associated distinct tremulousness, the likeness to delirium tremens is very pronounced; indeed, I believe that delirium tremens should be considered a form, or variety, of confusional insanity.

Very rarely ought there to be any trouble in recognizing the true nature of confusional insanity. The history of the attack, the knowledge that the outbreak was preceded by an

exhausting disease, traumatism, or emotion, the failure of bodily nutrition and of general nerve force, the lack of dominant emotional excitement, the stupor, the peculiar mental confusion, the kaleidoscopic character of the hallucinations, make diagnosis easy.

The prognosis in confusional insanity is favorable. Krafft Ebing states that seventy per cent. of his cases have recovered, and in my own experience, even when the mental confusion has amounted to complete and absolute imbecility, complete recovery has almost invariably occurred, provided that there has been no preëxisting organic bodily lesions, such as unsound kidneys or degenerated arteries. Death may, however, occur in complicated cases. If the mental recovery be not complete, the result is lack of mental power, but never a so-called partial reasoning insanity, never a state resembling that of paranoia.

Usually the first question to be settled, in the treatment of confusional insanity, is as to whether the patient shall be sent to an asylum or kept at home. The proper answer to this depends absolutely upon the pecuniary circumstances of the sick person. If there be abundant means to provide a sufficiency of trained nurses, it is much better to keep the patient in his own home. Nothing can be done in an asylum that cannot be as well done at home, and as the person returns to mental health the shock and depression of finding himself within the walls of an insane asylum may greatly retard or prevent convalescence. If, however, the pecuniary means be restricted, it is essential in severe cases to employ asylum treatment. Unless there can be an abundance of thoroughly trained nurses, there is little chance that the patient will be properly fed and cared for, and the feeding is of the greatest importance. The appetite is usually *nil*, but the powers of digestion are commonly much better than they seem, so that forced feeding is well borne. Again, not rarely the patient refuses food either in part or altogether, and then artificial feeding must be resorted to, which, of course, requires skilled attend-

ants. In all cases the administering of nutritious food, and even rich food, at short intervals, in as large quantities as the stomach will tolerate, should form the basis of treatment.

A point to which I think sufficient attention is not given is the maintenance of the bodily warmth of the patient; by overheated rooms, by hot-water beds or bags of hot water, and by the warmest clothing obtainable, every effort should be made, when the temperature is sub-normal, to restore it. An alarming, if not fatal, fall of temperature is liable to occur, and to be overlooked till the patient passes into a collapse which may be hopeless. The nurses ought, therefore, to be instructed to watch continually the temperature of the patient, and, in the absence of a physician, to institute the proper means of heating the body whenever the bodily heat falls below 97°.

Rest, massage, and exercise are three hygienic measures which must be carefully applied in every case. Their use is, of course, directed against the physical side of the disorder, and they are to be employed precisely as they would be in similar bodily conditions without distinct disturbance of the mental functions. I may be, however, pardoned in calling the attention of the Fellows of the College to the fact that in many of these cases the brain excitation distinctly masks the absolute exhaustion of the patient, so that the vital strength is much less than it seems: it happens that not rarely, especially in the earlier portions of an attack, even so gentle a form of exercise as massage is capable, if very vigorously applied, of intensifying rather than relieving the general weakness. Under these circumstances, absolute rest in bed with the maintenance of the utmost quiet is essential. Massage, carriage-riding, and other forms of inactive exercise may be employed as the strength returns, but active exercise of any kind is usually to be discouraged until convalescence has fairly set in. Sometimes, however, the lower nerve centres progress much more rapidly toward health than do those of intellectual life, so the general bodily strength increases without a corresponding growth in mental power; under such circumstances,

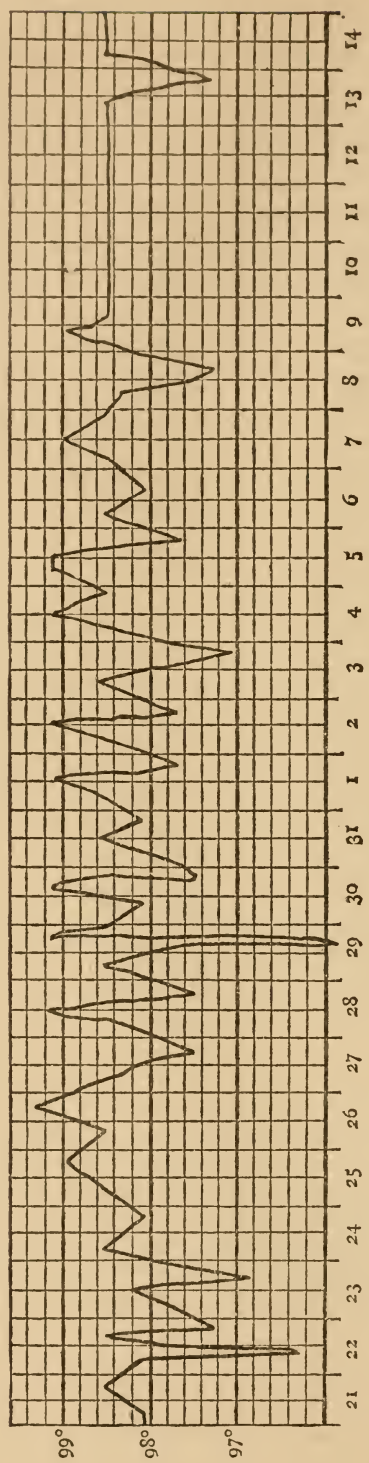
I have found removal from the home surroundings to the seashore, mountains, or other places of resort, combined with the encouragement of out-door exercise of a not too severe type, to be followed by the happiest results.

Drugs are to be employed with a twofold object. First, and chiefly, to increase the nutrition of the nerve centres and general tissues of the body; for this purpose iron combined with bitter tonics, administered in small or large quantities, according to the individual character of the case; strychnine given in ascending doses to the limit of physical tolerance, and phosphorus continuously exhibited in such small doses ($\frac{1}{120}$ of a grain to $\frac{1}{100}$ of a grain) as not to disturb digestion, I have found to be of great service.

Second, to obtain sleep when there is wakefulness, or to quiet delirious excitement. Here the bromides naturally suggest themselves, and they are often used in very large quantities; it should be remembered, however, that the bromides are powerful depressants, not merely to the functional, but also to the nutritive, activity of the nerve-cells, and I am sure that I have seen very distinct injury done by their free use in confusional insanity. In any individual case the selection of a hypnotic or quietant is to be based chiefly upon the results of trial. In some instances opium seems to act with service; more commonly hyoscine is of advantage. Chloral and sulphonal ought certainly to be tried on occasion. As a calmative, the hot-pack is often advantageous, and I have seen a very active delirium apparently greatly benefited by free blistering of the scalp.

Illustrative Cases.

Case I.—Mrs. S., aged thirty, had suffered before marriage from hysteria, insomnia, and other nervous diseases, due to severe fright. She was delivered of her second child January 3, 1888; during pregnancy she was feeble, and afterward continued very weak. January 29th, she was noticed to be very depressed and apathetic, and in the afternoon she insisted on stripping herself and going about the house naked. The next day she was distinctly, but not violently, maniacal.



I saw her first on February 1st; she was then mildly maniacal, talking incessantly and incoherently, not knowing where she was or recognizing the persons who were about her, and insisting that she was not at home, but at her father's house. Her pulse was very feeble, and her muscular system greatly relaxed. During the next two weeks the mental condition bordered closely on complete imbecility. At times there were hysterical emotional disturbances, as shown by crying and distress. Her physical condition was that of very pronounced weakness. During the day she was usually quiet and apathetic; but at night she was wakeful and talked incoherently, and was most of the time mildly maniacal, at times desiring to get out of bed. Only at rare intervals could she be made to understand at all that she was in her husband's house; but often in the day-time, when aroused from her habitual state of silent stupor or profound apathy, she would recognize one or two of the most intimate of her relatives. When not in stupor, she talked nonsense to herself for hours together, and at her best she answered the most simple questions incoherently without reference to their meaning. By the 21st of February her general bodily condition was greatly improved and her mental functions had so far returned that she knew her surroundings and appreciated her circumstances, her position, and her past illness; but the simplest thinking was performed with great feebleness and slowness, and the memory was still very imperfect. I did not see the patient after this time, but I am informed that she slowly convalesced, and by the latter part of April appeared to have assumed her previous bodily health and mental condition.

The temperature chart which is appended shows the remarkable irregularity of the temperature, with complete absence of anything like diurnal rhythm, and also that when the tem-

perature was high the exhibition of a dose of chloral or morphine was immediately followed by a fall. Tincture of digitalis did not affect the temperature.

On the 8th of January temperature fell distinctly below the normal, and through the night it continued to go down until about noon of the 10th, by which time the patient was in a state of alarming collapse, from which she was rescued by stimulants, external warmth, etc.

It should be further stated, that at no time was there uterine disease or any evidence of septicæmia, or of disease of other portions of the body or nervous system.[1]

Case II.—Mrs. A., aged seventy-three, seen in consultation with Dr. Shapleigh, October 10, 1889. About two weeks before had had the breast removed on account of cancer. The wound did very well, but one week after the operation delirium came on, manifesting itself at first at night. She waked up not knowing where she was, and tried to get out of bed, talking loudly, insisting she should go home, etc. After this she was more or less irrational up to the time of my visit; but the delirium was more active at night than in the day-time. In the day-time she could usually be made to understand where she was, but at night urgently pleaded to be taken home, and attempted to go home. At my visit she recognized her daughter, although at one time she knew no one, and seemed also to know Dr. Shapleigh, but answered questions incoherently. No report as to temperature could be obtained, except that once or twice when taken it was a little below the norm. She talked to me with some degree of rationality, but with very pronounced, evident mental enfeeblement, and was doubtful as to where she was. I visited her again October 22d; the general feebleness had improved; after my last visit she had had a spell of excitement and great disturbance, insisting that she had been visited by us in order that she might be taken away, that she was to be destroyed, and so on. Now talks rationally about old affairs, and has an indistinct remembrance of my previous visit.

Case III.—Mrs. C. L., aged forty-two, no neurotic history obtainable, cheerful woman, moderate beer-drinker; at sixteen years of age she suffered the loss of one ovary; at nineteen she married, and shortly afterward had two children. Emmet's operation for rupture of the perineum was performed September 8, 1889, after the patient had been repeatedly purged very freely with salts. The operation required three hours and a half. After the operation the temperature was 97.5°, but a few days afterward it rose to 98°. She was cheerful, but took very little food; the only medicine given her were bromides and chloral to overcome some restlessness at night. On the night of the 21st of September she awoke the nurse and told her that there were several people in the room, and that the walls of the room were receding each way, and was with the greatest difficulty convinced that

[1] This case I have reported in another connection. See Therapeutic Gazette, 1889.

she was wrong. The next morning she was stupid and dazed, and when spoken to concerning her actions during the night, she said she believed "she was not quite right." The disturbance was attributed to delayed menstruation; on the 23d her menses came on, and until the 28th she appeared rational: but it was noticed that she would get up at night and wander around her room. On the 28th of September the much-dreaded removal of the stitches was attempted, but before completion had to be abandoned on account of the intense alarm of the patient.

On the 29th she was very much depressed and worried, and declared that there were three people in bed with her. Hitherto she was anxious to get out of bed, but now after permission had been given her to get up she made no attempt to rise. Finally, she was persuaded to get up, but she walked very feebly and at this time could not feed herself or button her clothes. She now refused food from the nurse, expressed fear of her to friends, and constantly watched her. On the 2d of October it was noted that she no longer recognized her rooms or any of the familiar objects around her, and that she constantly pleaded to be taken home, it being impossible to convince her that she was in her son's house where she habitually lived. During the night of October 4th she attacked her nurse, who, clad in a bright red dressing-gown, was combing her hair by a low light. Shortly after this she talked freely of the horrible red object she had seen during the night, saying it was the devil, and the onslaught had evidently been made by her in self-defence. On the 4th of October I saw her in consultation with Dr. Clara Dercum, her attendant physician. I found her with a bewildered expression of countenance, feeble in body and mind, answering questions more or less incoherently, and recognizing only in a confused, very uncertain way that she was at home. She was put on the use of tonics, forced feeding, and a $\frac{1}{16}$ of a grain of morphine with chloral at night to obtain sleep. On October 9th she escaped from home, and went to her brother's house and stated that she had been turned out of doors, and that "he and her brothers were deceiving her." On being told differently she seemed to perceive her mistake, and was willing to apologize. On October 10th she was much stronger and had better appetite. On October 12th, the notes of the case say, "during the last few days Mrs. L. has been getting distinctly stronger, walks much better, feeds herself, and can button her own clothes. She still, however, remains uncertain in her talking, and shows marked evidence of confusion of thought and failure of memory. Thus, she is supposed to be reading a novel, but will read two pages for hours together just wherever the book happens to be open, without thinking of turning a leaf. Evidently by the time she has reached the end of the second page she has forgotten the one before it."

Nov. 1. "Mrs. L. is well. She states that much of the time during her illness she believed she was in a house of ill-fame and that her attempts to escape were based on this delusion."

CASE IV.—Mr. J., aged about fifty, controller of a large railroad, with a history of excessive work for a series of years, and also much recent worry. First applied at the office of Dr. A. G. B. Hinkle, with whom I afterward saw the case in consultation; at that time he had diarrhœa, feebleness, rose-colored spots, and other symptoms of walking typhoid fever, he apparently being at about the tenth day of the disease. He was at once put to bed and properly treated. He soon became somewhat delirious, with a maximum temperature of 104°, but by the close of the third week his temperature had fallen to 99°, and he seemed to be entering upon convalescence. On the twenty-sixth day maniacal symptoms began to develop, and continued to increase for a length of time. He was worse at night, and also had violent paroxysms in the day-time. He would become very active, and try to get out of the windows or out of the room, so that constant vigilance and care had to be exercised to prevent his injuring himself. His temperature was irregular, at times above the normal, but usually sub-normal, falling as low as 97.3-5°. The bowels were constipated, and there was retention of urine, followed later by constant dribbling; finally, there were involuntary evacuations from the bowels. When I first saw him his mental failure had deepened into complete imbecility, so that he could not be got to recognize any of his familiar friends or his surroundings, to express desire for food, or do anything that was rational. He was at times quiet and deeply apathetic, but much of the time he talked incoherently. He was very wakeful at night, and at such time especially prone to be actively delirious. His pulse was somewhat soft, his muscular strength excessively feeble, his tongue heavily coated. In his delirium he was always desiring to be taken home, and had, as a constantly repeated vagary, the belief that there were babies in the bed, and that they were liable to be hurt.

For some time treatment appeared to have no other effect upon him than to quiet his delirium and increase his physical strength, but by and by intervals began to show themselves in which he exhibited evidences of rationality, and could be brought to recognize, more or less indistinctly, those about him. There were still frequent paroxysms, often lasting for hours, in which there was complete loss of mental power with active delirium. Under the measures adopted his physical condition was all the time improving; he became so well that he went out riding, and grew altogether very much stronger, but his mental faculties did not improve as fast as his general health.

In the sixth or seventh week of his mental trouble, and about the tenth week of his whole attack, he was still subject to violent paroxysms of delirium, and on the night of the 25th of January seemed as insane as he had been at any time. He received the dose of morphine which he had for some time regularly taken at night, and awoke on the morning of the 26th with mind entirely clear, and has never since had a relapse.

In regard to the treatment of the case, the only points worthy of note

were the great benefit which appeared to follow blistering of the whole cranium, and the good effects which were had from the use of the hydrobromate of hyoscine ($\frac{1}{95}$ of a grain repeated every four hours until the fourth dose was taken). Sulphonal, urethan, chloral, and the bromides were tried as hypnotics, but without success, and, when the hyoscine lost its power, morphine was employed, seemingly with much advantage.

CASE V.—The following case, although not yet terminated, I append a note of because it is different in some respects from the ordinary type of confusional insanity.

Mrs. X., a woman of about forty-five years old, menstruation regular, no hereditary taint or neurotic history obtainable. She is stated to have been a very energetic woman, especially interested in religious matters. About six weeks ago, before my seeing her, she went to the country to visit a near relative who was dying of a chronic disease. She assisted with the nursing, losing many nights' sleep, and was also impressed with the belief that it was her special work in life to see that the sick person was converted, a belief in which she was confirmed by numerous persons about her. This was the cause of great excitement and excessive worry to her.

No history of distinct prodromes could be obtained, although her husband stated that she had been sleeping badly for some days previous to the outbreak, and had, perhaps, acted strangely. About one week previous to my seeing her she had suddenly become maniacal, talking incessantly and incoherently, and at times violently. There was almost complete sleeplessness, and from the description hallucinations appeared to have been present, as she talked very much about certain writings which she saw upon the wall. When I saw her (October 10, 1889), I could find no traces of hallucinations or delusions; but she was much excited, walking about the room a great deal, starting and acting strangely, talking incessantly in a very excited manner, and more or less incoherently, and was also entirely unable to give connected replies to simple questions. Most of the time she was in a condition of moderate emotional exaltation, but was liable to occasional brief pathetic paroxysms, in which without obvious reasons she would weep and be much distressed; usually, however, she was smiling and happy. She recognized that she was in her own home, knew her relatives, but at the first and subsequent interviews confused me with physicians whom she had seen in a distant city in reference to her sick relative; her memory was also evidently very much impaired. She gave no evidence of religious feeling, no signs of sorrow or care about her dying relative; indeed, all ideas connected with him or with her previous state of feeling seemed to have entirely passed out of her mind. At my various visits she did not appear to remember our intercourse. When I would go into the room, sometimes she would sit still, sometimes she would suddenly rise from her chair and confront me, and sometimes move off from me; her conversation being in this wise: "Good

morning, doctor—are you Dr. Jones, of Minneapolis?—Yes—No—did you get my letter I wrote?—Dr. Pepper, of Minneapolis, was a fine doctor—I am told, sir, best in the city—yes—Dr. Jones—best doctor—who are you, sir?—did you ever know me?—if you didn't know me, I didn't know you—yes—I—I—I—I know you—wrote you a letter—Dr. Jones, of Minneapolis—did I write you letter?—Dr. Jones—I believe, sir—Don't come near me, sir—Oh!" covering her face with her hands and beginning to cry.

DISCUSSION.

Dr. Charles K. Mills: While Dr. Wood's paper is an excellent presentation of the subject of insanity following acute diseases, both as regards the symptoms present and the treatment to be adopted, I do not see many points for discussion in connection with the paper itself. In the first part of it he rather advocated the idea that we should not subdivide our insanities into so-called diseases, taking the ground, perhaps, that we should look upon the pure insanities at least as a single disease or a simple series of symptom-groups. In a sense this is, of course, correct; but I think that it would be bad science and rather a step backward to advocate too far a doctrine of this kind. In the present state of our knowledge of insanity the only reasonable classification we can have, except for a few forms, is one based upon symptomatology.. The classification in a science like insanity is intended more for suggestion and stimulation to study and for purposes of treatment, than for anything else. It is not and cannot be based upon absolutely known facts with reference to pathology. I believe that it is well to recognize mania as a disease, or as a symptom-group which we label a disease, distinct from melancholia; and also in certain forms of mania and melancholia to indicate a distinct combination of these symptoms as circular-insanity.

If the views which I understood Dr. Wood to advance were carried out, they would sometimes mislead us. For instance, in reference to the bromides: there are certain so-called forms of insanity, such as mania in one or two of its types, which yield better to bromides, combined with other drugs, than to anything else; whereas in the conditions he particularly discusses bromides may be useless and harmful, while opium, tonics, and other hypnotics may be useful.

The whole tendency of modern thought and work is toward differentiation of symptoms, and to the erection of useful classes in the study of disease, even if we have no known pathological facts in certain forms; the mania, melancholia, or other symptoms which patients present indicate to us that while the same parts of the brain may be attacked, they are attacked differently. Take the two opposite conditions of mania and melancholia, and take simply the one great line of manifestations in the motor sphere. In the one there

is excessive motor excitability and in the other great torpor. Similarly with reference to other functions. I do not believe that all of these diseases are the result of cerebral exhaustion. Take, for instance, acute delirious mania: in certain cases there is a condition of true active congestion of the brain cortex and membranes, whereas in other forms of trouble, even with excitability, we have different conditions.

Dr. E. N. Brush (by invitation of the President): I did not expect, not being a member of the College, to be called upon to discuss this paper. I think, perhaps, from the way in which Dr. Wood has referred to mania, dementia, etc., that he has been misled into the belief that we are inclined to classify symptoms as diseases. Alienists, as a rule, recognize the fact that mania and melancholia are merely symptoms, and in some cases may be due to identical pathological conditions. These may be the results of anæmia; but I agree with Dr. Wood that they also occur with the opposite condition of active or passive congestion in exhaustion. I am in doubt whether all alienists would agree with him in considering post-febrile insanity, insanity of traumatism, insanity following puerperal conditions, and what has been classified as stuporous melancholia or primary dementia, as constituting one pathological symptom. It is certain with our present ideas of pathology that the man who received a blow on the head and presents symptoms of stuporous dementia, has not the same pathological condition as his sister, perhaps, in another ward suffering with puerperal mania.

The time has not yet come, and our knowledge of the pathology of insanity is not sufficiently advanced, to enable us to say that katatonia and some other forms are distinct diseases—that there is a pure insanity which may be considered as distinct a disease as typhoid fever. All alienists are, however, in unison in regarding the mental states, whether of excitement or depression, or the apparent absence of mental activity, as merely the symptoms or external manifestations of the pathological state of the brain. These symptoms are, however, by no means certain indices of what that state may be.

I agree with what has been said in regard to the improper use of bromides in some of these cases. The earlier symptoms of stuporous dementia are sometimes such as to lead the physician in ordinary practice to consider the case of a congestive type and needing sedatives, and the bromides are naturally given. This often retards the convalescence and embarrasses the physician who has the subsequent management of the case.

Dr. John Ashhurst, Jr.: I do not wish to take part in a discussion on mental diseases, but Dr. Wood has alluded to forms of insanity observed in surgical affections. Whatever the pathological condition may be, I think that he has not made a sufficient distinction clinically between the varieties met with in these cases. We are all familiar with the form of mental failure which sometimes precedes death from wasting diseases, such as cancer. Then, again, we have what Dr. Wood alluded to: the condition following injuries to the head, which seems to me to be quite distinct. Surgeons are

familiar with the temporary insanity or temporary imbecility which follows concussion of the brain, and from which, in the large majority of cases, the patient will recover if carefully and judiciously treated. In other cases, not properly treated, death may follow, and, in my experience, usually from abscess of the brain.

Again, we have what has attracted attention only in the last few years, and that is the form of insanity following operations, a condition which even acute observers have fallen into the error of considering the same as traumatic delirium, with which surgeons have been familiar for years. It is hardly necessary to say that it is wholly unlike traumatic delirium. I have seen two cases of what might be considered examples of insanity after operation, both following operations for hemorrhoids. One of them, while it presented marked delirium for a day or two, with delusions, might perhaps have been considered an aggravated case of hysteria, since it occurred in a young hysterical woman. In the other case, that of a middle-aged man, there was positive insanity with delusions, melancholia and great depression lasting two or three weeks, with occasional maniacal outbreaks. This patient, who ultimately made a good recovery, presented no symptoms for some days after the operation. I do not pretend to express any opinion in regard to the pathological condition present in these cases, but certainly the clinical features are very different in these three forms of mental affection occurring in surgical cases.

Dr. William Osler: On the question of prognosis in these cases, I should be glad to hear something more, as this is the point on which the friends are naturally most anxious. There is nothing more distressing than after watching a case of typhoid fever to convalescence to have mental symptoms of a grave form supervene. So far as my own experience goes, the prognosis is favorable. In the last fifteen months I have seen five instances—four after typhoid fever and one after pneumonia. All of the cases after typhoid fever recovered, though one, which was afterward under Dr. Chapin, recovered, I think, and returned to her home, but subsequently died. One case following pneumonia is still under observation. These cases are not very common. I remember to have seen but seven—two after pneumonia and five after typhoid fever. Six of them recovered.

Dr. Chapin: The subject of Dr. Wood's paper—"Insanity following Acute Disease"—is one of much interest. Insanity may be said to be of infrequent occurrence in hospital experience as a direct result of acute disease, as it does not happen in one per cent. of our admissions. Indirectly, typhoid fever, the cachexia that ensues upon exposure to marsh malaria, septicæmia that sometimes attends the puerperal state and surgical conditions, and the hardships and privations of military campaigns, are all factors in the production of insanity. I can well recall the large number of cases of insanity that occurred among the early emigrants returning from California, who were exposed often several days to the malaria of the Isthmus of Panama, and also

among the soldiers of the late war who were broken down in health, or those who had been prisoners of war. General ill-health, or invalidism, is a cause of fully thirty per cent. of our hospital admissions. In all of these cases insanity arises from that impairment of the nervous mass which results from deterioration of the quality of the blood, and as a consequence the nutrition of the brain falls below the normal standard. This seems to be the relation of acute and chronic physical disease to insanity.

The prognosis of insanity following typhoid fever and other acute diseases should be regarded as favorable, if it depends upon the above causes alone. If there has been a complication, in the nature of a meningitis, during the course of a fever, unless the products of the inflammatory process are wholly dispelled an unfavorable result may follow. In this connection it is important to call attention to the very frequent post-mortem evidences of the existence of meningitis at some period in the history of many cases of insanity. Doubtless it exists when not indicated by pronounced symptoms in a subacute form. If there is reason to suspect its existence, it is important, by means of prolonged treatment, to dissipate the exudations that follow inflammatory processes of the membranes of the brain, bearing in mind the intimate relation they hold to the nutrition of the brain. There exists in this direction a rich field in which to do valuable pathological work.

I would have been glad to have heard Dr. Wood's approval of the importance of a bountiful administration of nutritious food in the treatment of all forms of acute insanity, for in this respect I think an advance has been made. It is well known that many of these cases will take, and seem to require for their proper management, three and four times the quantity of food usually thought necessary for any able-bodied laboring man.

As to the effect of hospital residence and treatment, some here may not agree with all that Dr. Wood has advanced in his paper. It must be conceded that insanity has no bright side. If a patient has abundant means and can afford to turn his house into a hospital, employ nurses and physicians, it may sometimes be advisable to make a trial of this method; but the result to the patient and the indirect depressing influence upon a family cannot be always regarded as satisfactory. Of the result of private care of the insane in families we know but little in this country, as no official or other reports are made. In England, where official reports are made, thirty to forty per cent. of admissions to the county and organized asylums recover, while of those under private care nine per cent. recover. Krafft-Ebing is reported to have said in a recent address that the average of recoveries among the lower classes was fifty-six per cent., whereas in the higher classes it was only sixteen per cent., which he attributes to the fact that the former class enjoy to a greater degree the advantages of early asylum treatment. Admitting that a sentiment exists in the community, as Dr. Wood has stated, sentiment alone should not debar the patient from having the best chance to recover, or the physician from exercising his best judgment.

Dr. James Tyson: In the last year I have had an experience with three cases of mental derangement following acute diseases; all after typhoid fever, and all recovering. One of these cases, I have no doubt, is referred to by Dr. Wood, as he saw the case in consultation with the family physician and myself. This case, had it not been well nursed by a double set of nurses, would have been attended by serious results. On one occasion he would have jumped from the window and on another had almost gotten to the street. The duration of this case was such that we were on the eve of sending him to an institution, and it was at this period that Dr. Wood was called. He advised a longer delay, and the patient eventually recovered and is now in perfect health.

Dr. Wood: The ideas which have been brought forward seem to be sufficiently novel to attract discussion and some diversity of opinion, but much of the diversity which seems to exist has arisen because the gentlemen have not confined themselves to the subject of the paper. I did not speak in regard to the use of bromides in insanity, but in regard to their use in *confusional* insanity, and the difference which seems to exist between Dr. Mills and myself is mere seeming. Again, I did not speak of the asylum treatment in insanity, but to the asylum treatment in *confusional* insanity; and in such cases as that referred to by Dr. Tyson, I think that great damage would have been done by condemning the patient to an asylum. This gentleman was a superior officer of one of our greatest railroads. He was overworked, and contracted typhoid fever, which was followed by confusional insanity, but he recovered perfectly at home. Had he recovered in an asylum, he would have gone back to his work surrounded by a different atmosphere from that which surrounded him when he went back from an attack of typhoid fever. We have no right to expose a man who has the means to avoid it, to the stigma which attaches, although unjustly, to an asylum.

Let me say a few words in regard to the question whether all alienists think as I do, that these so-called diseases are symptom-groups. What I object to is that, by labelling these symptom-groups diseases, we are liable to have our thinking affected. Not long ago I wrote a paper on this subject which was reviewed by one of the most prominent alienists in this country, and in this review it was affirmed that the holder of such views must be one of those unfortunate individuals who were forced to become teachers before they had been students.

My paper did not refer to dementia resulting from traumatism of the head or to traumatic delirium, which occurs during the active stage of injury, but to those peculiar symptom-groups which develop when the patient is getting well from the surgical operation or the injury. All that I mean to indicate is that I believe that we have a group of affections having a common cause, namely, failure of the cerebral nutrition, with cerebral excitement, after various acute diseases. While septic delirium does occur after childbirth, it does not follow that all or even the majority of cases of puerperal mania are that character.

REMOVAL OF AN ENORMOUS ENCYSTED VESICAL CALCULUS BY SUPRA-PUBIC LITHOTOMY. RECOVERY.

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[Read November 6, 1889.]

John D., æt. fifty-five, from Renovo, Pennsylvania, was admitted to the University Hospital May 4, 1889, with the following history:

He had been troubled with symptoms of vesical irritability for twenty-six years, suffering severe pain during urination, chiefly at the end of the penis. He had always been a moderate drinker, usually of beer; he was a constant sufferer from dyspepsia.

Examination of urine: Specific gravity 1012, reaction alkaline, small amount of albumen accounted for by the pus present, no sugar. Microscopically: Abundant bladder cells, pus, and triple phosphates. No casts.

Sounding clearly demonstrated the existence of a calculus in the bladder, and gave the impression that it was of moderate size, as the peculiar click was only to be elicited when the point of the sound was turned in a particular direction. On May 8th the patient was etherized and a lithotrite was introduced into the bladder. Repeated efforts were made either to seize or to crush the stone, but were unsuccessful, the points of the instrument seeming to slide over the surface of the stone, and always without taking hold. I remarked to those present that I was dealing either with an encysted calculus, or with one of the largest size, or with both; that the stone was apparently a hard one, as not the smallest particle was brought out on the blades of the instrument, and that for these reasons the supra-pubic operation was indicated. This I at once proceeded to perform.

In the absence of a rectal colpeurynter, a half dozen soft sponges fastened to silk threads were inserted into the rectum. Ten ounces of boracic acid solution were injected into the bladder, bringing it well up toward the umbilicus. A piece of rubber drainage tube was tied around the penis to prevent the

exit of fluid. I then opened the bladder in the usual way, making a small incision immediately above the pubes. On dividing its anterior wall with the knife, the point of the latter impinged directly upon the stone, which was another evidence of its unusual size. On putting my fingers into the bladder I found it was nearly filled with an enormous calculus occupying the *bas fond*, and covered over at least three-fourths of its surface with folds of mucous membrane held tightly in place by strong bands of lymph. It was with great difficulty, and only by the exercise of considerable force, that I was able to strip this covering from the stone. I finally succeeded in doing so, using the pulps of my fingers so as to do as little violence as possible to the tissues. The wound in the bladder wall and that in the soft parts were enlarged upward, the stone seized with the forceps, and, finally, delivered. Two large drainage tubes were introduced through the wound into the bladder, and two superficial sutures were placed in the soft parts. The bladder was washed out every three hours with a saturated solution of boric acid with 1 : 5000 bichloride of mercury. The bichloride caused an erythema around the wound, and phenol sodique 1 to 10 of water was substituted. One drainage tube was removed May 22d, the remaining tube May 27th. A soft catheter was passed through the urethra and retained in position. On June 3d, the wound had almost healed and the catheter was removed. A small amount of urine found its way through the wound. On the following day the temperature rose to 101.8°. There had been no movement of the bowels for some days. The constipation was overcome by a dose of Rochelle salts, and the catheter was replaced June 7th. The temperature at once fell to normal, and remained so. No urine passed through the wound after June 10th.

On June 14th the catheter was finally removed, and the patient left the hospital entirely well June 17th. A careful examination of his urine at this time revealed nothing abnormal.

The patient made a practically uninterrupted recovery, although the length of time which the disease had existed, the close adhesions of the stone to the bladder walls, its enormous size, necessitating not only a larger wound than usual, but also causing considerable damage to the edges of the wound during the removal of the stone, all led me to feel great anxiety as to the result of the case. The stone weighed at the time of its removal nearly nine and a half ounces (4550 grains) ; its greatest circumference was eight and a half inches, and its transverse circumference seven and a half inches. The patient sat up on the twenty-fifth day, and the wound was entirely closed at the end of four and a half weeks.

I have to-day received a letter from him, in which he says that he has never been better in his life.

This case seems to me of special interest from three points of view :

1. The absence of fatal kidney disease in a case of such long

standing, and in the presence of such an extreme source of ureteral obstruction, frequent urination, etc.

2. The encysted condition of the calculus.

3. Its unusual size and weight.

I am quite aware that many cases have been recorded in which enormous calculi have been found after death in the bladder of patients who died from intercurrent disease, or from conditions not directly involving or dependent upon the urinary tract.

In the majority of instances, however, it is probable that when a stone attains a weight of more than two or three ounces, and is carried for any length of time, it produces a condition analogous to that which I found in the case of a patient from whom I removed post-mortem this calculus weighing 532 grains.

He came to me with a history of long-standing vesical calculus, which was easily confirmed on examination, and which also showed that the stone was probably of extremely large size; I therefore decided upon the supra-pubic operation, to which the patient consented. He was extremely weak, and had a profuse dysenteric diarrhœa.

The night before the day fixed for the operation he suddenly died, and the autopsy, which I conducted the following afternoon, showed a condition of the kidneys which made it marvellous that he had lived so long. The entire secreting structure had disappeared, and the organs were transformed into multilocular sacs containing enormous quantities of muco-pus. The ureters were dilated to almost the calibre of the small intestine. The bladder contained a calculus weighing 532 grains. The origin of this dilatation of the ureters and kidneys in such cases is of great interest, as no appreciable obstacle to the entrance of urine into the bladder is usually discoverable. It seems probable that in some instances the dilatation is brought about by the frequent contraction of the walls of the bladder, each act of this character temporarily interrupting the flow of urine through the vesical end of the ureter. This, taken in conjunction with the extension of inflammation from the bladder by continuity, probably explains the condition. An operation would have been certainly fatal, and I was thankful to have escaped the additional risk of death during the use of ether. The urine was so

loaded with vesical débris that the condition of the kidney could not be ascertained with any certainty.

A brief review of the history of some of the cases in which excessively large stones were operated upon will show at once that the patients were almost invariably in a condition which rendered surgical interference surely and more or less rapidly fatal. As Keyes says: "Surgical works on this subject teem with rare and curious cases of calculi of great size and weight, the largest of which will be found to have been taken from dead bodies, and the next in size pretty uniformly to have brought about fatal results by their removal during life."

Among examples of unsuccessful attempts to extract large stones may be mentioned the case operated upon by Vitellius and recorded by Fabricius Hildanus, in which the stone weighed twenty-two ounces.

Geyer details one in which the fragments weighed ten ounces.

Charles Preston mentions one taken from a priest in 1690 and preserved in the Charité Hospital of Paris. It weighed fifty-one ounces, but the patient died before the operation was completed. Borellus describes one of eighteen ounces extracted by Quesnotus. Marteau removed one of fourteen and another of twelve ounces. Mr. Birch extracted one of sixteen ounces from a man in St. Thomas's Hospital; Deguise one of thirty-one ounces. Graefe, of Berlin, extracted one of twenty-one ounces; LaMotte, Vidal, and Eller all mention examples of twelve ounces. In all these cases the patients died in from one to ten days. In a case of Sir Astley Cooper's, in which the stone weighed sixteen ounces, the patient survived only four hours; and in the case of Mr. Dalyrymple's, of Norwich, in which the operation was abandoned before its completion, and the patient died in three hours, the weight of the calculus was upward of twelve ounces. Mr. Erichsen reported a case (*Lancet*, 1885) of a primipara in whom a large tumor was found obstructing the outlet of the pelvis, and necessitating craniotomy. A month later a stone was removed by the vesico-vaginal operation. It weighed five and a half ounces, and measured eight inches in the long, and six inches in the short circumference. The patient died in eight days, and at the autopsy extensive disease of the kidneys

was found, the right being converted into a mere cyst, while the left was in a state of chronic pyelitis with great dilatation of the ureter.

Sir James Earle reported (*Lancet*, 1853) the case of Sir Walter Ogilvie, Baronet, of Dundee, who, at the age of twenty-three, became paralyzed from the effect of a blow on the back. Twenty years later Mr. Benjamin Bell diagnosed stone, and recommended extraction, but the operation was declined. At the age of fifty-three the patient's sufferings became so violent that he determined to have it extracted.

In consultation with Mr. Cline, the operation of supra-pubic lithotomy was thoroughly considered, and concluded to be "uncertain and dangerous because the bladder, thickened and exquisitely irritable, could not bear to be further distended with fluid, and the stone, although so large, had not raised it sufficiently high to obviate the danger of wounding the peritoneum and penetrating into the cavity of the abdomen. The usual lateral operation was, therefore, judged to be the only safe and probable means to be attempted." This was accordingly performed, but it was found impossible either to seize the stone in the forceps or to crush it and extract it piecemeal. The patient died ten days later, and a stone was found weighing forty-four ounces, and completely filling the bladder and the bony pelvis. Earle was at one time shown a calculus weighing eighteen and a half ounces, extracted by Cheselden. The patient died the following morning.

Mr. T. Holmes has recorded (*Trans. Path, Soc.*, London, 1870, vol. xxi. p. 267) the case of a calculus weighing twenty-five ounces, removed, after death, from the bladder of Sir Thomas Adams, a baronet, who died at Sprowston Hall, near the city of Norwich, during the reign of Charles II. The stone was of uric acid, and measured twelve and a half inches in its longitudinal circumference. Sir Thomas was able to be up and about until he met with an accident. He died at eighty-one years of age. The stone is now in the museum of St. Thomas's Hospital, London, of which institution he was president about the year 1662.

In the museum at Cambridge there is a thirty-two ounce calculus, taken from a woman who died about the same time. Mr. de Morgan describes two large calculi now in the Canterbury Museum, removed

after death from the same patient, and weighing together twenty-two and a half ounces.

Mr. Holmes has also reported (*Trans. Path. Soc.*, London, 1874, vol. xxv. p. 181) a case in which unsuccessful attempts were made at both lithotrity and lithotomy, and the patient died the day following the latter attempt. A stone weighing 3620 grains was found filling the bladder and the prostatic urethra. It was 3.8 inches in length, 2.8 inches wide, and 2.6 inches thick. After all the soft parts had been cleared away from the pelvic bones, it was found just possible to push this stone out from the pelvis through its lower outlet.

Mr. Robert Hardey records (London *Medical Gazette*, 1829, vol. iii. p. 569) the case of a man sixty-six years of age, from whose bladder he, after death, extracted a stone weighing twenty-seven and a half ounces avoirdupois, its greatest circumference being fifteen and a half inches.

Sir Henry Thompson reports (London *Lancet*, 1857, vol. ii. p. 474) the case of a patient, forty-six years of age, from whom, after death, a calculus was removed weighing twelve ounces, and measuring nine and a half inches in its transverse diameter, and ten and a half inches in its greatest longitudinal diameter. The nucleus was composed of uric acid, but the external layers contained urate of ammonium and phosphatic laminæ. He had had urinary symptoms since he was fifteen years of age. Sir Henry Thompson remarked that when he came under his care, about nine months before his death, the idea of extraction was not entertained because the stone was found to fill the bladder completely. In a case recorded by Mr. Williams, a uric acid calculus, which weighed twenty-five ounces, and measured ten and a half inches in its short circumference, and twelve and a half inches in its long circumference, was removed, after death, from the bladder of a gentleman eighty-one years of age.

Dr. Augustus Browne records (*Trans. Path. Soc.*, London, 1868, vol. xix. p. 277) the case of a man forty-two years of age, who had symptoms of stone in the bladder for twenty-five years, having been afforded relief only by drinking an extraordinary mixture of gin and beer. He died with symptoms of uræmia. The bladder was found contracted upon three calculi, the total weight of which was one and

a quarter pounds less twenty grains. Externally they seemed to be composed of triple phosphate.

Charles Monad has reported (*Bulletin de la Société de Chirurgie*, Paris, 1881, p.762) the removal of a stone weighing 1950 grains, by supra-pubic lithotomy; in which case, the patient having nearly recovered, but a small fistulous orifice remaining unhealed, one or two injections of tincture of iodine were used, being thrown into the small suppurating cavity situated behind the abdominal wall and communicating with the fistula. This was followed by erysipelas, and the patient died on the fortieth day after the operation. The same surgeon records another case in which he extracted a stone 90 millimetres in length, 75 millimetres in width, and 58 millimetres in thickness, weighing 346 grammes, or about 5200 grains. The patient died on the fifth day.

These cases sufficiently illustrate the history of the exceedingly large stones which have been mentioned in surgical literature as having been found after death, or as having been the object of unsuccessful surgical attack during life.

2. The second point to which I have alluded is the incarceration of the stone by means of such firmly organized lymph and by such a projection of the mucous membrane around about it that it practically lay in a cavity of its own, communicating with the bladder by means of the aperture through which a portion of its convex surface protruded. The mode of formation of these cysts containing calculi is variable. In the majority of patients of the age of this one, it has always seemed to me probable that the condition was brought about primarily by enlargement of the prostate causing vesical hypertrophy with distention, and with the protrusion of the mucous membrane between the fibres of the detrusor, which gives rise to the well-known "sacculated bladder" of old people. Gross mentions a case in a man of eighty-four where such a sacculus held a gallon.

These cysts thus formed have no muscular fibres, and, of course, no power of completely evacuating their contents, although they may do so partially by aid of the abdominal muscles. A certain amount of "residual urine," therefore, always remains in them, decomposing, throwing down a sediment, and setting up a catarrh, the mucus of

which binds together the crystals and sedimentary deposit, forming the nucleus for the stone. In this instance, however, the symptoms date so far back into middle life (beginning when he was twenty-nine years of age) that this explanation hardly seems entirely satisfactory, and it appears more probable that the cyst was formed by the gradual sinking of the stone as it grew, into a pocket made by its own weight, the hypertrophied and thickened mucous membrane rising above its borders and becoming fastened to it by lymph-bands.

According to Keyes, an encysted stone is either partially within the bladder and partly encysted, the neck of the cyst forming the obstacle to removal, or the orifice of the cyst is so small that to all intents and purposes the calculus is extra-vesical.

For the former set of cases, if the stone be of moderate size, and if the condition be discovered during a perineal lithotomy, after the part projecting into the bladder has been removed, or before, in some cases, the operator endeavors with his finger-nail, or a director, a searcher, a scoop, or other elevator, to dilate or slightly tear the neck of the cyst and work out the calculus. In this manœuvre, pressure upon the hypogastrium or through the rectum may be of great assistance. If moderate manipulation fails to dislodge the stone, a curved, probe-pointed, long bistoury may be used with great caution upon the finger as a guide, to cut moderately the constricted edge of the neck of the cyst in one or more places. Such use of the knife involves risk, and it rests with the operator to decide according to the circumstances of each case whether to assume the risk, or to resort to supra-pubic section.

In the case of a large pouch connected with the bladder, and containing a small stone, the neck of the pouch may be dilated with the finger or small forceps, and the stone reached.

When the stone is practically outside of the bladder, or in a dilated ureter, some operators prefer to leave it untouched. Thompson acted in this way in the presence of a stone in the ureter. It is plainly the surgeon's duty to cut scantily in such cases.

That the incarceration of a calculus adds greatly to the difficulties of its extraction has always been recognized by lithotomists.

Mr. James Miller, Professor of Surgery in the University of Edinburgh, described (*Monthly Journal of Medical Sciences*, 1848,

p. 574) the removal of two stones, one of which the size of an egg, was easily extracted, but the second was so encysted that great difficulty was experienced in seizing it, and the soft parts adherent to it had to be carefully stripped from it with the tip of the forefinger before it could be freed and removed. The patient died about a month after the operation from Bright's disease and pyelitis, and an abscess of large size was found in the right kidney.

Various proposals exist for dealing with encysted and sacculated stones. Littré advised seizing hold of the stone and cyst in the way described, and using it roughly, with the object of bruising the soft parts so as to cause suppuration in them, and disintegration of them; and with the hope of the stone (if not itself broken) becoming loosened during the suppurative stage, dropping into the bladder, and thence being extracted at another time.

Garengeot, with a bistoury, divided the neck of the sac, and then used forceps. Peyronie, Le Dran, Marechal, and others, have trusted to catching hold of the projecting part of the stone, and pulling violently, so as to detach it from its adhesions. Desault employed a concealed knife, or "coupe-bride," or "kystotome," for dividing the cyst. Among the older writers some have advised the stone to be undisturbed; in hopes that the wound would heal, and that the patient would be left little worse, if no better, than when he was found.

Scheutzer (*Medical, Chirurgical, and Anatomical Cases*, London, 1758) details the case of a clergyman sixty years of age, who for twenty years had felt a pain in the urethra with some obstruction in making water. Stone being diagnosed the operation for "its removal was performed according to the great apparatus." The operation was extremely tedious and difficult, but the stone was finally removed, after more than an hour's efforts. It was encysted and projected from the bladder into the prostatic urethra. It weighed five ounces. The final result is not stated.

Dr. Newbigging reported (*Monthly Journal of Medical Sciences*, 1848, p. 690) a case of encysted stone in which the adhesions had to be stripped from it with the finger in the same manner before its extraction could be effected. The stone was of large size but its weight was not given. The patient recovered.

Mr. Fergusson reported (*Transactions of the Pathological Society of London*, vol. vii. p. 84) a case in which a small stone having been recognized and crushed, the patient died twenty days later. A stone the size of an orange was found at the autopsy in a cyst at the right side of the bladder immediately behind and above the prostate gland. The opening communicating with the cavity of the bladder was about the diameter of a sixpence. The patient had suffered with urinary symptoms for twenty-six years. Mr. Fergusson remarked that the specimen was interesting from the great size of the stone, from the length of time it probably existed in this situation, from the slight irritation it had caused, and, lastly, from the rareness with which calculi are found in this precise locality.

Mr. Henry Lee presented to the Pathological Society of London (*The Lancet*, March 29, 1862, p. 328) a calculus for the removal of which median lithotomy had been performed. The stone was found to be impacted and immovable and could not be grasped by the forceps. Portions were broken off and at length were removed by the scoop. They weighed altogether nearly four ounces. The patient died three or four days afterward.

Mr. Erichsen reported (*The Lancet*, 1853, vol. i. p. 56) a case of encysted calculus in a patient who had had severe symptoms of stone in the bladder for several years. On several occasions Mr. Erichsen and his colleagues had heard the characteristic sound while the patient was being examined in the wards, but when he was placed upon the operating table, they failed to find it. He died about three years from the time he first came under observation, and a cyst was found upon the right side of the bladder containing two calculi, but the opening of the cyst was only large enough to admit the point of a middle-sized catheter. Mr. Erichsen congratulated himself upon having adhered to the rule never to cut for stone unless the presence of the calculus shall have been distinctly made out by percussion with the staff a few moments before the operation. He adds: "The case which we have this day to bring before our readers offers a beautiful illustration of the soundness of the above-mentioned caution handed down to us by our forefathers, for the post-mortem examination showed very plainly that though there was really a stone in the bladder, none of the means at our command could have

accomplished its extraction even with a very large incision." Like most of the authors I have quoted, he does not seem even to have taken into consideration the supra-pubic operation.

Mr. W. Cadge, in an article on sacculation of stone in the bladder (*The British Medical Journal*, October 2, 1875, p. 418), has called attention to the dangers of lithrotrity in such cases resulting from an injury to the walls of the bladder during the crushing, or the attempts at crushing.

3. As to the unusual size and weight of the calculus:

The stones larger and heavier than this which have been successfully extracted are very few:

Le Cat has recorded, 1774 (*Philosophical Trans.*, London, vol. xi. p. 238), a successful operation on a boy, twelve years of age, of a removal of a stone which filled the entire bladder, but does not give either its weight or dimensions.

In 1818, Charles Mayo, Esq., of Winchester, operated upon a man, aged twenty-eight, and extracted a stone of fourteen ounces and two drachms, avoirdupois; it measured eight inches and a half in its smallest circumference, by rather more than ten in the largest, and broke into several big pieces in the attempts to extract it.

Mr. Dickinson, of England, successfully removed, from a man of sixty-two, a calculus of a globular shape, composed chiefly of phosphate of lime, and which weighed eleven ounces. It broke into several fragments, which were taken away piecemeal. The operation was followed by sloughing of the rectum, and when the case was reported, several months afterward, a small fistulous opening still existed between this cavity and the bladder.

Mr. Earle, in his "Remarks on the Danger of Extracting Large Calculi" (*Medico-Chirurgical Transactions*, London, 1821, vol. ii. p. 69), says: "The following are the largest successful cases which I have met with: one fifteen ounces in weight, and four and three-quarters by three and one-half inches in diameter, extracted in 1746, by Mr. Harmer, of Norwich, related in Gooch's *Surgical Observations*, p. 54. The patient survived, but the wound never healed." One of thirteen ounces thirty grains in weight, three and a half inches in diameter, and eight inches in circumference, operated on by Klein, with perfect success (*Chir. Oper.*, p. 32). Klein likewise

refers to a case related in Mursinna's *Journal für die Chirurgie*, vol. vi. p. 94, in which a stone of twelve ounces and two drachms in weight was successfully removed.

Ambrose Paré mentions the case of a confectioner operated on in 1570, by John Collo, where the stone weighed nine ounces and was three and a half inches in diameter. (*Lib.* 25, *ch.* 19.) Tolet mentions a case of a stone ten ounces in weight and three and three-quarters inches in diameter, which was happily extracted, and the patient was recovering from the effects of the operation when an abscess formed in the kidney from the presence of another calculus; the patient died on the ninth day after the operation.

Of the prodigious number of cases on which Cheselden operated, the three largest stones successfully extracted weighed twelve, ten, and eight ounces. Mr. Earle adds: "Without citing any more authors, it will be sufficient to state that, as far as my researches into this subject have hitherto led me, very few instances of success are recorded where the stone exceeded seven or eight ounces."

Sir Astley Cooper, in an article on "The Size of Human Calculi" (*Lancet*, 1826, vol. i.), said: "The largest stone I ever saw extracted without its being broken (at the same time the patient surviving), is one that was taken from a person at the Norwich Hospital; it weighed eight ounces."

A very remarkable case of supra-pubic lithotomy, though hardly to be classed with the successful cases, occurred in 1837, in the practice of Professor Uytterhoeven, Surgeon to St. John's Hospital, Brussels. A man, aged thirty-nine, a native and resident of Brussels, had labored under vesical disease nearly twenty-seven years, when he was lithotomized. He survived the operation eight days. The calculus, which weighed upward of two pounds, was of gourd-like figure, and was accurately moulded to the shape of the inside of the bladder, being nearly seven inches in length, upward of four inches in breadth at the broadest part, and nearly two inches and a half in thickness. It had a rough, tuberculated surface, and appeared to consist of a number of thin, friable lamellæ. The walls of the bladder were indurated, and an inch thick, except at the point corresponding to the summit of the concretion, where they were completely worn down to the peritoneal coat. They contained several

purulent depots and fistulous passages. An immense abscess occupied the left iliac fossa. The small intestine and the mesentery were highly inflamed.

Dr. Rae has reported (*British and Foreign Medical and Chirurgical Review*, 1844) the case of a man, forty years of age, who had symptoms of stone for thirty years. The bladder was opened through the perineum by the usual method; the finger introduced into the bladder felt the stone, and an attempt was made to seize it with the forceps. This was found to be impossible, on account of the firm contraction around it of the thickened coats of the bladder.

The patient was much exhausted, and it became necessary to desist after an hour and forty minutes. The wound was kept open, and seven months later Mr. Fergusson crushed and extracted the stone, which weighed nearly eight ounces. The patient recovered.

Mr. Bullen, of Ipswich, records (*Lancet*, 1848) a case of the removal of a calculus weighing seven ounces from a girl, fourteen years of age. It was broken into many pieces before it could be removed. The patient, at the time it was reported, was progressing well.

Dr. Dudley reports (*Southern Medical and Surgical Journal*, 1850) a case of a youth, fifteen years of age, from whom a calculus weighing nine ounces, and measuring eleven inches in its greatest circumference, was successfully extracted by the lateral operation. The extraction was followed, however, by sloughing of the bulbous portion of the urethra, together with the whole of the accelerator urinæ muscles and a large portion of the perineal connective tissue.

In a table of ninety-six cases of vesical and urethral calculus operated on by Professor George E. Post, of Beirut, Syria, the largest weighed 1875 grains, or less than four ounces, and the case resulted fatally. (*Medical Record*, 1877, p. 469.)

In 131 cases of stone in the bladder operated on in South Carolina, and collected by Dr. Francis L. Parker (*Transactions of the South Carolina Medical Association*, 1878, p. 37), the largest stone removed in one operation weighed eight ounces, and the next four and a half ounces; none of the others were over two ounces.

Dr. C. T. Gardner (*Medical Record*, vol. xiv., 1878) removed a calculus which weighed twelve and a half ounces, measured in its

several diameters three and three-eighths inches and two and one-quarter inches, and had for a centre a small oxalate calculus, the periphery being made up of phosphates. The operation for its removal was commenced with the lateral section, as the instruments on hand were insufficient to crush it *in situ*. The patient, a man of forty-nine years, did well after the operation, and a rectal fistula which formed from sloughing closed spontaneously.

Dr. Mettauer, of Virginia, is said to have removed a calculus weighing sixteen ounces by the lateral operation (Eve's *Surgical Cases*), but so far as I know, no details of the case have been published.

Gross and Agnew each allude to a case in which Dr. Dunlap, of Ohio, removed a stone of twenty ounces after fragmentation. The patient lived three years after the operation. After quoting some of the above-mentioned cases, Gross adds:

"Although the above cases clearly show that a stone, even of large size, may occasionally be successfully extracted, yet it is equally certain that they must be regarded as so many exceptions to the rule, rather than as the rule itself. Most generally, indeed, the patient dies either from exhaustion during the operation, or from the effects of inflammation a short time afterward."

A very few words will suffice as to the diagnosis and operative treatment of such a calculus as this. A stone of this size and shape, freely movable in the bladder, could undoubtedly be seized by the lithotrite and measured in its small diameter, but it is hardly probable that it would be caught in the direction of its length in the one examination which it is customary to make for the purpose of measurement before deciding upon the particular operation to be performed. If the bladder were much contracted, it could not be grasped in its long diameter at all. The existence of a cyst-wall nearly covering it, greatly reducing the area over which the characteristic click of the searcher is obtained, preventing the lithotrite from seizing the stone with any firmness, and rendering it almost immobile, of course adds greatly to the difficulty of diagnosis. Bimanual examination, with a finger in the rectum and another over the hypogastrium, might be of use in persons with very attenuated abdominal walls, but even this method would scarcely convey an accu-

rate conception of the dimensions of the calculus. It is probable that the true size of such calculi will generally be discovered in the future as in the past—during the performance of the operation intended for their removal.

As to the choice of that operation there can scarcely be two opinions, supra-pubic cystotomy being so evidently the only available method, and the one to be employed at once when, as in this instance, the character of the stone is disclosed during the attempt at litholapaxy.

It would, perhaps, be more useful to consider the duty of the surgeon who, having opened the perineum by either the median or lateral section, finds that he is dealing with a calculus of this description. Under these circumstances, the abandonment of the perineal route being imperative, the high operation should be performed, either at once, if the patient is in good condition and well fitted to bear shock, or, if that is thought inadvisable, as soon as the perineal wound has fairly well granulated, the patient then having the benefit of the drainage by that route without the danger of septic absorption.

DISCUSSION.

Dr. J. Ewing Mears: The lateness of the hour renders it impossible to discuss at length the propositions presented in the interesting paper read by Dr. White. Lodgment of a calculus in the wall of the bladder and its fixation in position by means of adhesions forming the variety described as encysted calculus is, I believe, sufficiently rare to make its occurrence of interest—especially when it is found to exist in connection with a stone of the size and weight of that exhibited to-night. It can readily be understood how a calculus of such weight may form a pouch for itself and remain fixed in position without becoming adherent or attached, as in case of small stones.

Of the various methods of accomplishing enucleation described, that which effects dislodgment by laceration of the adhesions and evulsion with the finger is to be preferred as efficient and freer from danger than any which involves the use of the knife or other instrument.

Dr. William Hunt: I was under the impression that we had in the Mütter Museum some stones that would nearly match this in size, but I have been unable to find any. At the Pennsylvania Hospital we have a specimen

of a bladder with an encysted stone which was taken from a man who died of cancer of the stomach and who also had prostatic stone. I operated, at his request, simply for euthanasia, and to his great relief. He lived in comparative comfort for a long time. I removed a number of stones from the prostate, but several still remained encysted in the bladder walls.

It seems curious, when we see and hear of such cases, that some authorities should say that encysted stone is simply the excuse of the unsuccessful lithotomist.

ON A NEW GRAPHIC CLINICAL CHART.

By J. P. CROZER GRIFFITH, M.D.,
PHYSICIAN TO THE ST. AGNES'S AND THE HOWARD HOSPITALS; ASSISTANT PHYSICIAN TO THE HOSPITAL OF THE UNIVERSITY OF PENNSYLVANIA.

[Read November 6, 1889.]

How entirely unsatisfactory the usual method of recording pulse and respiration is, may be seen at once on inspecting one of the ordinary charts, such as are in use in many of our hospitals. Take, for example, a carefully plotted temperature tracing of a rather long-continued case of typhoid fever. Somewhere at the side or bottom of the chart, and comparatively out of sight, are one or two columns of figures, the result of a long and careful observation of pulse and respiration. A glance at these figures tells us absolutely nothing as to the course which the pulse and respiration have pursued during the disease, and one cannot but wonder whether it was worth the trouble of recording them. Certainly the busy physician hesitates before reading these columns through with that care which is demanded in order to gain any information from them. If the attempt be made, one reads a short distance and notices, perhaps, a pulse-rate which seems higher than those preceding it; then, some distance further along, another rather high rate is observed; and the reader is obliged to look back in the effort to find the first figure, now forgotten, in order to make a comparison. Clinical charts made on this plan are often worse than uninstructive—they are actually harmful. They tend to teach us to exalt the value of clinical thermometry at the expense of observations on the pulse and respiration, particularly the former. A recent German writer has deprecated the habit of noticing the temperature chart immediately on enter-

ing the sick-room, and of failing to study the character of the pulse; and believes—and rightly, too, we must admit—that in this respect our methods are inferior to those of our predecessors. Those who are still sceptical—if any can be sceptical in this matter—and who think that the value of observations on the pulse is always less than that of temperature records, should take to heart the words of Wunderlich, the great exponent of the indications of temperature in disease. "In general," he says, "that which affords us the best standard is the circumstance [*i. e.*, either pulse or temperature] which appears the most unfavorable. With rapid pulse and moderate temperature, the former; with slower pulse and higher temperature, the latter—and this rule will be found most applicable when the contrast between the two is greatest."—*Medical Thermometry*, New Sydenham Society Translation, 1871, p. 214.

Granted, then, that observations on the pulse and respiration are often as valuable as, and sometimes more valuable than, the record of the temperature, it becomes all-important that these be recorded in a manner which will at once impress on the eye and mind the daily changes which may take place. The graphic method with the use of a plotted line having been found the best for temperature, there seems to be no good reason why this should not be applied to the pulse and respiration also. I do not, of course, intend to claim that the recording of pulse and respiration is always, or even usually, as important as that of the temperature. The respiration can certainly be ignored in many diseases, and even the state of the pulse is frequently a matter of no moment whatever. My only claim is that they often become far more worthy of attention than the temperature, and that in all cases where it is deemed best to note them at all, the record should be kept in the proper manner—*i. e.*, graphically.

With this idea in view, two or three medical friends, in company with myself, devised some five or six years ago a small chart folding to the size of an ordinary prescription blank, and intended to be carried in the pocket. It was arranged to last for eleven days, a record being made twice daily. On it the pulse and respiration were to be plotted in the same manner as the temperature. Three colors—red, blue, and black—were used in printing, and red, blue, and black lines, or dotted, dash, and continuous black lines were employed to

designate respectively the pulse, respiration, and temperature. Though all three started from the same normal line, no attempt was made to preserve a pulse-respiration-temperature ratio.

My own experience with these little charts was very favorable, and I have now on file a number of them, very interesting in many particulars. Some of these show beautifully the high pulse at the outset of scarlet fever; the rapid pulse and respiration in pneumonia, with the sudden fall of these with the descent of the temperature at the crisis, etc. The chief objection to them consisted in their small size and their limited range, unfitting them for anything but pocket charts.

Though having the production of an improved chart often in mind, I took no further steps in the matter until after the publication of Dr. Handford's article on "The Temperature-pulse-respiration Ratio in Health and Disease," in *The Practitioner* for November, 1888. The author depicted a clinical chart more nearly according with the plan I had entertained than anything I had previously seen, though there were several important points in which it seemed defective. By personal correspondence, and through the courtesy of Dr. Easter of the University Hospital, who has been greatly interested in the subject, I have been able to examine many clinical charts from various countries, including England, France, Austria, Germany, and Russia. Some of these I exhibit to-night, and you may be surprised, as I was, to find how very general has been the effort to represent the pulse and respiration as well as the temperature graphically, showing the general conviction that this is the proper method. Many suggestions have been gathered from these, and the attempt has also been made to avoid what seemed to be errors or inconveniences in them.

The chart which I exhibit to-night (see plate)—and in which, it is but right to say, I have no pecuniary interest whatever—is the result of a somewhat extended study of the matter; and, though it is by no means perfect, I hope it may commend itself through some real advantages. I have already intimated that it is unnecessary and even undesirable with this chart to record pulse and respiration in every case. Both may be omitted, and the temperature curve alone be plotted; or only the curves for temperature and pulse, or for temperature and respiration, may be drawn, as the needs of the case demand.

The size of the new chart is 8½ by 11 inches, rendering it suitable for use either in hospitals or in private nursing. The left-hand margin is nearly 1 inch wide, to allow for binding. The paper is sufficiently thick to permit of full notes being made on the back, if this be desired. At the top of the chart are places to record the name, age, and residence of the patient; the diagnosis; the date upon which the record is commenced; and the case-number. These seem to me to be all the facts worthy of record, apart from the case-history, which would be kept on a separate sheet in hospital practice, or on the back of the chart or elsewhere in private nursing.

Immediately below these data are horizontally extending spaces for the day of the month, day of the disease, and time of day. "Morning" and "evening" have been omitted, thus allowing records to be conveniently kept every two hours, if desired, or at any time when special observations are made. For this purpose a horizontal line divides in half the space for the time of day. The hours at which records are made after 12 at night, up to and including 12 noon, would thus be written above this line as indicated by the printed letters "A. M.," while other records would, of course, be made below the line. I am quite convinced, from my own experience, that this omission of all printed stated times is a distinct advantage.

At the lowest portion of the chart are similar horizontal spaces divided by vertical lines into sections for the recording of the number of movements of the bowels—indicated by strokes as they occur; the condition of the urine, and the weight of the patient—so important a matter in many cases, and one which is, whenever possible, determined weekly in the medical wards of the University Hospital. The space allotted to the urine is again divided into three smaller portions, preceded respectively by the words "oz.," "contains," and "sp. gr." (specific gravity). The object of these needs no further explanation.

The largest portion of the chart remains to be considered; the part destined to receive the plotted pulse-, respiration-, and temperature- tracing. The rectangles for this purpose have been made of such a size as to allow space on the chart for a three-weeks' record,

with two observations a day, and to permit sufficient upward and downward range to the variations.

One criticism can be made with great justness—namely, that it would have been much better had the hair lines been replaced by faintly dotted ones. I was, indeed, very anxious to have this done, but found that it would increase the cost of the chart so greatly that its usefulness would be much curtailed. The publisher promises me that, should the chart prove a success, this alteration shall be made.

The dark horizontal line marked "Normal line" indicates the average normal rate of pulse, respiration, and temperature. To determine what the normal is, was a matter of no small difficulty. After consulting a large number of standard text-books, I have adopted the figures 70 for the pulse, 18 for the respiration, and 98.4° for the temperature.

At the left of the chart are three columns of figures printed respectively in red, blue, and black ink, and indicating respectively pulse, respiration, and temperature. The normal line intersects these columns at the figures mentioned. It will be observed that the remaining figures above and below these hold a certain relation to each other, indicating the *pulse-respiration-temperature ratio.* To determine this ratio was a matter of even more difficulty than to fix the normal line. The pulse-respiration ratio is usually assumed to be as 4 : 1, but the pulse-temperature ratio receives but little notice in the text-books. Liebermeister made a long series of investigations on that subject, and arrived at certain conclusions; Wunderlich gives other figures for the relationship between pulse and temperature; and Aitken differs somewhat from both. Handford assumed his scale arbitrarily, starting with a normal of pulse 64, respiration 16, and temperature 98.4°, and ascending and descending with a pulse-temperature ratio of 10 : 1, and a pulse-respiration ratio of 4 : 1. He has had a very extended experience with his own charts, and finds this ratio very fairly correct, but, if anything, a trifle too low, as the lines for pulse and respiration fall decidedly above that for temperature. As it seemed to me that 64 and 16 were too low for the normal pulse and respiration, I have placed these at 70 and 18, which would have the effect of lowering

the pulse and respiration curves, as compared with those in Dr. Handford's charts. For this and other reasons I have made the pulse-respiration ratio 4 : 1, and the pulse-temperature ratio 10 : 1. It is, of course, evident that there are many fallacies in the adoption of any ratio whatever, since the rate of the pulse and respiration, especially of the latter, vary so greatly at different ages and under different conditions. These facts do not, however, militate in the slightest degree against the necessity of clinical study of pulse and respiration, and the graphic method of recording them; and in doing this it is necessary that some ratio be adopted as the one most constantly observed.

It will be noticed that the space allotted to the time of day and that for the days of the month and of the disease are each of the same height as the divisions of the chart corresponding to one degree of temperature, while that containing the name, disease, etc., is twice this height. Consequently, in cases in which pulse or respiration are unusually rapid, the lines indicating these can be extended upward as far as need be.

A few words on the method of recording the observations—though an explanation seems scarcely necessary. In the upper left-hand corner of the chart are the brief directions, instructing that the pulse shall be drawn either with red ink or pencil, or with a black hyphen line; the respiration with pale blue (or, perhaps, violet would be still better) ink or pencil, or with a dotted black line; and the temperature, as usual, with a continuous black line. Pulse and respiration are to be indicated by red and blue dots connected by lines of the proper color, exactly after the usual plan of marking temperature. If, for instance, the pulse at one observation is 100, say at 8 A.M., a dot is placed opposite the red figure "100" in the column indicated by the hour of 8, which has been written in the proper space for "Time of day." If the next record is 120 at 6 P. M., a second dot is placed opposite "120" in the column indicated by 6 P. M. The two dots are then connected by a red line—or, if more convenient, by a line of dashes. The different tracings will, of course, often intersect, but the differences in the manner of marking them will prevent the possibility of any confusion. Should it happen that the dots for temperature and either, or both, pulse and respira-

tion fall on exactly the same spot, only one (the black) need be made, as the colored lines which run to it will show at once the correct position of the different tracings. At the extreme right side of the chart is a centigrade temperature scale for convenience of reference, and at the extreme left, much as in the chart of Dr. Wilson, are the words "Clinical Notes" above, and "Treatment" below. Small arrows here indicate a region where brief notes may be entered, if desired. Much writing on the chart is, however, best avoided, as tending to interfere with the plotting, and to produce confusion. Only those notes—if any at all—should be made which have a direct bearing on the appearance of the tracing—such, for example, as "10 grains antipyrine," "chill," "sweat," etc., if the temperature has experienced a sudden fall; and "much excited," or the like, if the pulse is out of all proportion high. Other and fuller notes can better be recorded on the back of the chart or on a separate history sheet.

These charts have been made and are now for sale by P. Blakiston, Son & Co., of this city. They sell at $7.50 per 1000, or with an additional charge of 50 cts. if it is desired to have the name of the hospital printed at the top. They are also sold in packages costing 50 cts. and holding 50 charts.

GRAPHIC CLINICAL CHART.

Designed by J. P. Crozer Griffith, M.D.

MARK
PULSE ——— OR - - - -
RESP. ——— OR .....
TEMP. ———

Name, ... *Age,* ..... *Residence,* .....

Disease, ... *Case No.* ..... *Date,* .....

DAY OF MONTH.

DAY OF DISEASE.

TIME OF DAY. A. M. / P. M.

Centigrade Scale.

43 42 41 40 39 38 37 36 35 34

PULSE	RESP.	TEMP.
170	43	
	42	108
	41	
160	40	
	39	107
150	38	
	37	106
	36	
140	35	
	34	105
130	33	
	32	104
	31	
120	30	
	29	103
110	28	
	27	102
	26	
100	25	
	24	101
90	23	
	22	100
	21	
80	20	
	19	99
70	18	NORMAL LINE
	17	98
	16	
60	15	
	14	97
50	13	
	12	96
	11	
40	10	
	9	95
30	8	
	7	94
	6	
20	5	

Clinical Notes.

Treatment.

URINE. *Oz.* / *Contains* / *Sp. Gr.*

BOWELS.

WEIGHT.

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ON CALF-PEPSIN, AND ESPECIALLY THE GLYCERITE OF CALF-PEPSIN, AS AN ADJUNCT TO A MILK-DIET.

By FRANK WOODBURY, A.M., M.D.,
HONORARY PROFESSOR OF CLINICAL MEDICINE IN THE MEDICO-CHIRURGICAL COLLEGE OF PHILADELPHIA, ETC.

[Read November 6, 1889.]

For a number of years, in treating cases of apepsia in infancy, and as an adjunct to a milk-diet in a later period in life, I very frequently used rennet-wine, which was acceptable to the patient, and which I believed had some peptonizing power. I also occasionally met with a communication in the medical journals reporting favorably upon its employment in the digestive disorders of infants. The experiments of Scheffer, published in 1870, in the *American Journal of Pharmacy*, however, demonstrated very conclusively the fact that an alcoholic menstruum was not suited to the preservation of digestive power in a pepsin preparation, and that glycerine diluted with water was the best of all menstrua for this purpose. Last winter in conversation with a prominent pharmacist of this city, who has had considerable experience in the manufacture of the wine of rennet, I suggested that a solution made with glycerine would be more acceptable and would prove to be a more active digestive agent than the alcoholic preparations. After several consultations and experiments we produced this Glyceritum Pepsini Vitulini, which I have the honor of presenting to the attention of the College this evening. The method followed in its manufacture is as follows: The stomachs of healthy young calves are taken and washed clean with running water, the mucous membrane is dissected off, crushed, and treated with glycerine and water slightly acidulated, in which it remains

until the pepsin is all extracted; the solution is then filtered carefully, and to this is added a small proportion of the acid phosphate of lime. This was considered a useful addition because cow's milk is slightly deficient in this constituent as compared with breast-milk, and in order to insure a slightly acid reaction to the finished product. Scheffer found in the experiments already referred to, that calf-pepsin requires the presence of an acid medium in order to enable it to exert any digestive action upon albumin.

As milk is increasing in favor, not only as an easily obtained and easily assimilated form of nourishment, ordinarily, but also as a therapeutic agent of no mean value—in inveterate indigestion, in gastric ulcer, in diabetes, renal disease, typhoid fever, in inanition, as an important part of the rest-cure, and in the perennial baby-feeding problem—the conditions affecting its administration, both favorably and unfavorably, deserve consideration. Quite apart from the results of the intentional or accidental adulteration, and variations in the quality of the milk, which have been frequently discussed in the journals, there may be objections raised by the patient himself, often arising from defective digestive capacity, that may prevent the use of milk, which otherwise would be appropriate to the case. Bartholow has pointed out some of the abuses of milk-diet in therapeutics, and very properly advises against its administration in acute rheumatism, especially in large quantities, on account of the excess of lactic acid introduced into the system by its digestion, which he believes increases the risk of endocarditis, delays recovery and favors the occurrence of relapses. He also states that "In certain disorders of the digestive functions, milk causes a sense of discomfort, decided uneasiness, oppression—sometimes even pain—and it prolongs the morbid condition. The cases of this kind may be grouped into two classes: those in which the casein is the offending material, those who cannot properly digest the cream or butter. We find examples of the first class more frequently among children, but they are by no means uncommon in adults. They are detected the more readily in early life, because the curds are rejected by vomiting, or appear undigested in the stools. Adults unable to digest casein, or who digest it slowly, or painfully, have epigastric distress, heaviness and oppression for several hours after meals, stupor and disinclination for exertion

coming on after an hour or two and continuing until the offending material has passed well down the intestines." The remedy for digestive disorder occasioned by excess of cream is generally applied, with quite unusual consideration for the patient's welfare, by the milk vender before delivering the article to the customer; when this has been neglected, the excess of cream may be removed by partially skimming the milk before giving it to the patient. This procedure will generally be required in the case of Alderney milk, which often contains too much fat even for infants.

When, on the other hand, there is experienced more or less difficulty in disposing of the casein (and this generally occurs in fever, on account of the deficiency of secretion from the peptic and other glands of the stomach), the administration of pepsin, or an artificial gastric juice, seems the natural resource. Where the diet is cow's milk, it seems reasonable to assume that calf-pepsin, being the proper digestive ferment for this food, ought to be a valuable aid to the treatment, to prevent the occurrence of *embarras gastrique* and to favor the assimilation of the albuminoids of the milk. The dry calf-pepsin, I believe, has been entirely superseded by the more active pepsin of the hog. Scheffer found that it had only a moderate action upon boiled egg-albumin as compared with the usual form; and probably hog-pepsin when carefully prepared is preferable for most purposes where a digestive ferment is needed. But, where the diet is cow's milk and not hard-boiled eggs or meat, the calf-pepsin would seem to possess especial appropriateness.[1]

Without extending this communication unduly by going into unnecessary detail, I would simply make the following claims, which appear to me to be well founded, for the solution of calf-pepsin in glycerine which I have just presented:

1. The glycerite of calf-pepsin, prepared in the manner described, is a permanent, reliable, and elegant pharmaceutical preparation; beside being the most active form in which to administer this agent.

2. It differs from the wine of rennet in possessing greater coagu-

[1] See also article on "Glyceritum Pepsini Vitulini: a Form of Digestive Ferment Especially Adapted to the Treatment of the Condition of Apepsia in Young Infants," by the writer, in the Dietetic Gazette, July, 1889.

lating and peptonizing power; a drachm readily acting upon a pint of milk. Rennet, as stated by Randolph, will not coagulate boiled milk; the glycerite of calf-pepsin does so readily, and forms a soft delicate curd, consisting largely of peptones. In the feeding of infants this property becomes of special importance, in view of the prevalence of the use of sterilized (boiled) milk. In this connection we recall the statement of Kirchner that the difference in digestibility of milk from the cow and that from woman is wholly due to the large amount of peptones in the latter.

In the case of infants, I usually direct that the pepsin preparation (gtt. x–xx) shall be given fifteen to twenty minutes after feeding; in adults it is usually added (f℥ss to ℥j) to the milk before it is swallowed simply as a matter of convenience. Junket made with this glycerite of calf-pepsin, from either the unboiled or boiled milk, is a delicate and easily digested form of nourishment for the sick.

I would mention incidentally in conclusion that this preparation may be used to brush over the dry, coated tongue of typhoid fever patients; and to spray the throat in cases of diphtheria; although as yet I have not used it in this way. I can, however, speak confidently of its value in conjunction with sterilized milk in artificially fed infants; and as a useful adjunct to the milk-diet treatment of various disorders of the stomach and nervous system.

[1] Journal of Reconstructives, vol. ii. p. 6, iii. p. 10.

ANALYSIS OF FIFTEEN MONTHS' ABDOMINAL WORK IN THE GYNECEAN HOSPITAL, PHILADELPHIA.

By JOSEPH PRICE, M.D.,
OF PHILADELPHIA.

[Read December 4, 1889]

The analysis here given is offered to show the character and frequency of complications in abdominal and pelvic surgery, and not its pathology. For this reason the classification is arbitrary. Most of the cases have been reported in Society transactions, and the specimens exhibited, so that I will make no detailed report of them here. The list includes sixty-four consecutive operations, done within the past fifteen months during my service at the hospital.

None of the operations were done for hysteria, menstrual or hystero-epilepsy; none for pain or subjective symptoms, unless objective lesions were clearly recognized. Some of the cases came into my hands from my dispensary service, while others were referred to me in my private practice by men of large experience and good judgment. Many were very ill when I took them in hand. None of the cases were operated upon without fully explaining, so far as it was possible, the general nature of the operation to which they were about to be subjected, its reasons, its advantages, its dangers, with honorable carefulness to friends and patient. Too many cases go on the operating table, told that nothing short of an operation will cure them, but without being told the nature, object, and results of the operation. This has happened many times to both single and married patients. That this should be done, can only be explained on the supposition that an explanation will scare away the patient and an operation be lost. Operators so eager for work as this would indicate involve themselves in the extremest contradictions, as the

records of one of our societies will show. One operator has cited cases to show that pain was the only reason for operative interference in some of his most successful cases, while anon he urges that it would be better if pain *per se* were never considered a sufficient indication for operation. Such enthusiasm were better directed to neglected cases where grossest lesions are present and the sufferers are dying untouched.

Nearly every case here reported has been in some institution, undergone one of the many so-called conservative forms of treatment, running from simple blisters to the cautery, actual and electrical, while they suffered the tortures of the lost in a fools' paradise. Many had repeated attacks of localized peritonitis, while in nearly all the disease had been of long standing, the suffering being constant and great.

The comparative difficulty of abdominal and of pelvic surgery ought not to pass unnoticed. In its complications and their variety, the latter is by far the more trying. In abdominal adhesions an enlarging of the incision will generally offer a commanding point for the difficulty. This is not the case in pelvic adhesions, where often a conglomerate mass is met, to see which will not benefit in the least, and to handle which is impossible to a greater degree than can be accomplished by two fingers.

I will now notice some of the complications in detail, and also the technique of certain operations: In six cases I have operated for the removal of the appendages owing to fibroid disease of the uterus. The complications here were numerous, involving many of the ordinary forms of ovarian and tubal disease, with common ovarian cyst. The indications were two-fold: primarily for local disease, and secondarily to control advancing fibroid enlargement. The early removal of the appendages in this condition is one of the most successful operations in pelvic surgery. The recurrence of bleeding in these cases is perhaps less than in any other form of disease for which the operation is done, and the relief of suffering is notable.

In the advanced form of uterine myoma hysterectomy is to be considered. The safest and surest method at the present is that which deals extra-peritoneally with the stump. So far as results are concerned this is, by all odds, the ideal method. The reasons for this superiority, briefly stated, are these: The pedicle or stump

is absolutely under control as it undergoes shrinkage; it is entirely outside of the abdominal cavity; it can be treated safely by the dry method, with no possibility of infection, thus minimizing all danger. The causes for mortality in the intra-peritoneal treatment are: the large size of the pedicle; its vascularity; its tendency to shrink, and its liability to slough. The patients thus treated are prone to die of hemorrhage and sepsis. I have had nine consecutive hysterectomies, treated extra-peritoneally, without a death. My only death in hysterectomy was in a case in which there was concurrent cancer of the bowel.

I have had in this series fifteen cases of double pyosalpinx. In all these the adhesions were marked, implicating variously the intestine, bladder, omentum, and the sigmoid cellular tissue. The adhesions vary from dense organization to a friable, cheesy condition, easily broken down. The abscess tracks met in this condition are most various. They are sometimes single, oftener multiple, to as many as three or four. Herein appears the non-scientific character of attempted drainage by the vagina. In no other condition are we so certain of remedying the disease by direct interference and direct drainage. The method *par excellence* is to extirpate the abscess cavity or cavities and thoroughly cleanse and drain the pelvis, with the assurance that in the vast majority of cases recovery is certain. In five cases I met pyosalpinx, thus demonstrating that the double condition is most frequent. It is my practice, in dealing with single pus tubes, to remove both, knowing that in many cases there is specific cause for the disease, which will eventually necessitate a second operation if the present advantage is not seized upon to prevent it. The recognition of pus in the pelvis is not difficult. With a little special training, it can be determined generally with sufficient precision to leave no doubt as to the advisability of operation.

There is no treatment for pus but the knife. The earlier the operation is done the better. It is especially to be urged owing to the undoubted tubercular tendency generated in patients so affected. This has long ago been demonstrated by Bernütz and Goupil, and I have repeatedly called attention to it in my own work. I have repeatedly arrested the progress of such disease by operation.

In five cases double hydrosalpinx was met, and in three single

hydrosalpinx. The adhesions here were such as have already been referred to above. Broad ligament cyst, ovarian abscess, and hematoma were also met as complications of this condition. The number of ovarian cysts was seven, the adhesions involving variously the intestine, omentum, and uterus. There was one dermoid cyst. There were two cases of double acute salpingitis, with dense and extensive adhesions. The number of chronic cases of salpingitis was eleven. In these the double-sided lesion by far preponderated, with occlusion of the tubes and dense adhesions.

I twice operated for extra-uterine pregnancy; both times after rupture. In both the disease was bilateral, and in both it was simply suspected, because the lesion was bilateral. In one case there was hydrosalpinx of the opposite side. In the other the extra-uterine pregnancy was probably double: the existence of tubal and ovarian disease predisposes these women to extra-uterine pregnancy. In two of my cases I found a tubal pregnancy on one side and a hydrosalpinx on the other; Bantock found pyosalpinx on one side, and a tubal pregnancy on the other; Edis found a small ovarian cyst with tubal disease on one side and a tubal pregnancy on the other. Other well-authenticated cases might be quoted. Such cases as these must show the utter uncertainty of positive diagnosis. There is but one treatment, American or English; this is early and absolute removal of the murderous mass.

Under what may be classed as miscellaneous operations there was one supra-vaginal hysterectomy, complicated with double hydrosalpinx, cyst of the left broad ligament, and cystic ovaries. There were no adhesions. There was one peri-renal cyst, one parovarian cyst, one ventral hernia, a bi-cornuate uterus from which the appendages were removed, one exploratory incision, with a case of double simple ovarian cyst with dense adhesions. Blood cysts are very common, as also effusion of blood into the tubes. These are not ectopic pregnancies, notwithstanding the fact that macroscopically they greatly resemble it.

More than sixty per cent. of the cases were drained. The after-treatment was simple, and wholly in the hands of myself and a young, specially trained nurse. Anodynes were administered in three cases only, where the morphine-habit already existed, contracted in a general hospital while suffering for unrecognized trouble. All of the

cases recovered without sequelæ; no sinuses formed, and no ligatures have ever been discharged, notwithstanding they were applied to unhealthy structures. Many greatly emaciated and greatly exhausted patients speedily recovered good health.

Such results can only be attained by the utmost painstaking and care. The work is commonly difficult, always liable to be so; and only perfection of methods, exact detail, entire simplicity, and constant care can yield success. This success is often the greatest and only adequate recompense for a work which in the extent and magnitude of its complications surpasses all other surgery, and which, the better it is understood, the more it will be relegated into the hands of the few, out of the hands of the many, for its perfect accomplishment.

A. *Removal of both appendages for fibroid uterus,* complicated by—

a. Dense universal adhesions and retroversion (pelvis-bound)	1
b. Dense universal adhesions and retroversion (right hydrosalpinx, left acute salpingitis, and double ovarian cystoma)	1
c. Dense universal adhesions and retroversion (double chronic salpingitis and left ovarian cyst)	1
d. Dense universal adhesions and double chronic salpingitis	1
e. Chronic salpingitis, cyst of left ovary, blood cyst of right ovary	1
f. Double ovarian cysts (large)	1
	6

B. *Double pyosalpinx,* complicated by—

a. General friable adhesions	6
b. General healthy adhesions	1
c. General healthy adhesions (especially to sigmoid), cellular tissue abscesses both sides, general peritonitis	1
d. General peritonitis	1
e. Ovarian abscess one side, dense bowel adhesions	1
f. Ovarian abscess one side, dense bowel adhesions (large peritoneal cyst)	1
g. Ovarian abscess one side, dense bowel adhesions (necrotic patches on tubes, purulent peritonitis)	1
h. Ovarian abscess one side, dense bowel adhesions (patches on bowel necrotic through to mucous coat)	1
j. Ovarian abscess one side, dense retro-peritoneal cyst	1
k. Blood cyst one side, cheesy, friable adhesions—omentum, bladder, uterus, appendages and intestine fused into one mass	1
	15

C. *Pyosalpinx, one side* (one side removed), complicated by—

a. General firm adhesions, involving tube, omentum, and intestine, post-puerperal 1

b. General firm adhesions, multiple cellular tissue abscesses, peritonitis (other side removed fifteen months previous for pyosalpinx) 1

Both sides removed—

c. Ovarian cyst same side, broad ligament cyst opposite side . 1

d. Ovarian cyst same side, broad ligament cyst same side, occluded adherent tube opposite side 1

e. Ovarian and multiple cellular tissue abscesses same side, salpingitis and adherent ovary opposite side, peritonitis . . 1

5

D. *Double hydrosalpinx*, complicated by—

a. Dense bowel adhesions 2

b. Dense bowel adhesions and double multilocular ovarian cysts . 1

c. Dense bowel adhesions and ovarian abscess, one side . . 1

d. Dense bowel adhesions and broad ligament cyst, one side . 1

5

E. *Hydrosalpinx, one side*, complicated by—

a. Firm adhesions 1

b. Firm adhesions and adherent ovary same side, blood cyst of other ovary 1

c. Firm adhesions and adherent ovary same side, cyst of other ovary, and soft myoma of uterus, pregnancy suspected . 1

3

F. *Ovarian cysts, one side*, simple 2

Complicated by—

a. Salpingitis same side, firm healthy adhesions to uterus, omentum, and intestine 1

b. Adherent occluded tube and ovary opposide side (both sides removed) 1

c. (Blood cyst.) Both tubes tuberculous and adherent (both sides removed) 1

d. (Large multilocular cyst.) Rupture (post-puerperal), peritonitis 1

e. (Dermoid cyst.) Other ovary cirrhotic, general adhesions . 1

7

G. *Double acute salpingitis*, complicated by—

a. Dense healthy adhesions, retroflexed adherent uterus, cyst of one ovary	1
b. Dense healthy adhesions, retroflexed adherent uterus, cystic ovaries, pelvic organs, omentum, and intestine matted together	1
	2

H. *Chronic salpingitis, tubes occluded, dense adhesions*—

Condition on one side only	1
Condition on both sides	5
Complicated by—	
a. Retroflexed adherent uterus	1
b. Retroflexed adherent uterus and cystic and cirrhotic ovaries	2
c. Cystic and disintegrated ovaries	2
	11

I. *Ectopic pregnancy*, complicated by—

a. Rupture and hydrosalpinx of opposite tube	1
b. (Probably double) rupture of right tube, contents discharged in abdomen; left tube distended to size of sausage, with clot protruding from pavilion: placenta and membranes near cornual end	1
	2

Miscellaneous—

Supra-vaginal hysterectomy (fibroid uterus, double hydrosalpinx, cyst of left broad ligament, cystic ovaries, no adhesions	1
Parovian cyst, one side	1
Peri-renal cyst, retroperitoneal	1
Ventral hernia	1
Bi-cornuate uterus, rudimentary appendages, adhesions	1
Exploratory	2
Double simple cysts of the ovaries, firm adhesions	1

DISCUSSION.

Dr. William Goodell: I wish to congratulate Dr. Price on his admirable success. I desire also to endorse his statement with reference to extra-uterine fœtation—that is to say, that salpingitis often exists on the opposite side. In two cases on which I successfully operated this was found to be the case. In a third case the unimplicated tube and ovary were sound.

I can also confirm his remarks in reference to the success following removal

of the ovaries for fibroid tumor. It is remarkable what power this has in arresting the growth of the tumor and in checking hemorrhage. There is in this room a gentleman who sent a patient to me from Missouri, about two years ago, with a fibroid weighing at least fifteen pounds. I removed the ovaries, and there has since been a great diminution in the size of the tumor.

I am somewhat at issue with the reader of the paper in regard to the treatment of the pedicle of fibroid tumors. If it is a supra-vaginal hysterectomy, I should treat the pedicle by the extra-peritoneal method; but if it were a simple hysterectomy, viz., the removal of a pedunculated fibroid, I should not do so. I have had in the last two years six or seven cases which I have treated by dropping the pedicle, and have been successful in every case. One tumor weighed over fifty pounds; another over forty pounds, and a third over thirty pounds. The pedicles were as large as my wrist. I transfix and ligate provisionally, then remove the tumor, and scoop out the pedicle so as to make it funnel-shaped. I next untie the ligature, wind its ends around two forceps handles, and tighten it with all my strength. Then I stitch together the edges of the pedicle. The cases all recover without delay, for the process of recovery is simpler than when the pedicle is treated extra-peritoneally.

Dr. E. C. Montgomery: I unfortunately did not hear all of the paper. I must endorse what the author says in regard to the removal of pus in the pelvis. In a number of cases I have tapped and drained by the vagina, but several of them have subsequently required operative interference after the drainage had been maintained some time, and the exudation had changed from a serous to a purulent one. There was considerable difficulty in removing the mass—possibly more than would have been encountered if the operation had been performed primarily, A case recently operated upon was that of a colored woman who had been in the hospital about two weeks with pelvic exudation. This was opened *per vaginam*, and half a pint of serous exudation escaped. A drainage-tube was introduced, and for a few days there was improvement. Later the temperature rose, the discharge became purulent, and night-sweats supervened. The sac was removed by laparotomy. It was found adherent to the intestine to such an extent that in separating it the bowel was torn. This was at once closed. The opening in the vagina was so large that for several days the fluid used in irrigating passed into the vagina. The subsequent progress of the case has been all that could be desired. This case illustrates the importance of abdominal incision rather than trusting to vaginal drainage.

Dr. Barton C. Hirst: I should like to record a recent experience, which I think should modify Dr. Prices's edict that operation should not be done for pain alone. Ten days ago I operated on a lady on whom oöphorectomy had been performed some time ago. Since the first operation there had been pain increasing in severity. At the time I operated I was unable to establish any objective sign of trouble. I found that in the original operation one-

third of the ovary had been left, and around it were four tight ligatures. I removed the remaining portion of ovary and the ligatures, and since then the woman has been entirely free from pain.

Dr. S. Weir Mitchell: We have heard a good deal with reference to surgical interference in these cases. I should like to say a word in regard to surgical non-interference. In the last fifteen years I have seen a good many cases—certainly over twenty—where the ovaries have been condemned and a surgical operation decided upon, and yet I have seen these cases get entirely well without surgical interference. Putting aside a good many of these positive lesions described by Dr. Price, I cannot help thinking that there have been a good many ovaries taken out that should have stayed where Nature put them.

I will mention one illustrative case. Some years ago I was asked by Marion Sims, of New York, to see with him a girl, aged seventeen years. He told me that both ovaries were enlarged, and he thought one adherent. She suffered greatly at the menstrual periods, so much so that at times she was nearly insane. After trying a great variety of treatment he had determined that the ovaries should be removed. After going over her case carefully, I told him that I should like to treat the case for a short time. He was most glad to place her under my care, and I had the pleasure of returning her to him in three months perfectly well. She has since married and has three children. I could cite similar cases, but I mention this one because the authority for the removal of the ovary was such as all of us respected.

Dr. Price: I am scarcely familiar with the class of cases of hysterectomy to which Dr. Goodell has referred. I alluded to large, oedematous, rapidly growing myomas, and to cystic degeneration in large fibromas.

I think that I am right, and that Dr. Mitchell also is right. I agree with him in everything that he has said. I have myself never been guilty more than once of removing the ovaries for subjective symptoms. I operate for disease. In Dr. Hirst's case there was an offending organ. This was not a case of pain in a healthy organ.

The great question involved is whether or not the permanent result is satisfactory in hysterical women, referred to by Dr. Mitchell. In the cases I now report it is fair to say that the operations have been done to save life. Many of the patients had suffered from one to fifteen years. In bleeding fibroids my experience has been that we cure both the hemorrhage and the pain.

The operation in hysterical women is a failure. I believe that this is almost universally acknowledged. So far as my observation goes, no matter what the character of the suffering may be, the removal of healthy ovaries and tubes in no way benefits these women, except temporarily. Such operations throw discredit on all pelvic operations, particularly in the class under discussion on which we are called upon to operate to save life.

There is another point to which I desire to refer. I am satisfied that we occasionally have a death from opium. Three times in the last year I have taken from patients from one to three drachms of morphia. Last Sunday I operated on a patient who, after the operation was placed in a fresh bed. She insisted on keeping her pocket-book, and I took three drachms of morphine from under her pillow. She had morphia concealed in the corner of the handkerchief she carried to the operating table, and notwithstanding my precautions, she had an overdose of morphia a few hours after the operation.

Dr. Mitchell: I agree with Dr. Price in regard to this operation in hysterical women. I have seen many who had both overies removed and were none the better for it, and these seemed to constitute the most irremediable of all the cases of hysteria. I wish to make a single exception to the general statement. There is one condition which I think makes it justifiable to operate on healthy ovaries. That is where a woman, past the age of probable marriage, is insane only at the menstrual period. In two such cases removal of the ovaries was done at my request by Dr. Goodell. One was a case of homicidal mania at the menstrual periods, and the other case was one in which were developed extraordinary sexual appetites in a woman at other times modest. In both of these cases the recovery was absolute. I have watched both for years.

THE ABUSE OF UTERINE TREATMENT THROUGH MISTAKEN DIAGNOSIS.

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[Read December 4, 1889.]

While the treatment of uterine disease received a great impulse from the writings of Simpson in England, of Kiwisch and Scanzoni in Germany, and of Huguier and Rècamier in France, it is to Bennet that we chiefly owe the common use of local applications and the popularization of the speculum. The former gynecologists wrote about the more strictly surgical diseases of women, which were, and will ever be, relegated to specialists. But Bennet as early as 1843 in French, and 1845 in English, published his work on *Inflammation of the Uterus*, of which the last edition was issued in 1861. In it he contended that inflammation of the cervical canal is the main factor in female diseases; that from it come ulceration, displacement, leucorrhœa, menstrual derangement, and ovarian disorder; and that the inflammation, being limited to the cervical canal, can readily be dislodged by strong caustics. Written with dogmatic zeal, advising a definite treatment to a reachable and a visible object—a treatment which the humblest yeoman of the profession could carry out—few books of modern times have exerted so great and so widespread an influence. That riddle of the Sphinx—the cause of woman's ill-health—had been read and interpreted! The profession was taken fairly by storm; the effect was as electric as the appeal of Maria Theresa to the Hungarian nobles. From Arctic to Antarctic oceans, from the Mississippi to the Volga, countless speculums of varied form leaped from their bags and flashed in sunlight boreal,

tropical, and meridional. Cauteries, actual or potential, applied to actual or potential uterine sores, became the order, in fact, the ruling passion, of the day. In the general enthusiasm it was soon forgotten that every mucous membrane secretes, and that it must, of course, give evidence of its own secretion, just as every nose contains its mucus. Hence, every examined womb, being found to contain mucus, was as it were, invalided—that is to say, it was put on the speculum list. Naturally, then, not a womb being found healthy from the speculum standpoint, this much-abused organ was charged with being the cause of almost all the ills that female flesh is heir to, and it was treated—that is to say, maltreated—for every imaginable disorder that could not be referred off-hand to some other organ. Bennet, the medical Frankenstein, had evoked a monster which could not be curbed, and for five and thirty years the speculum ran riot.

But in this age of unrestful progress, at every turn of the hour-glass of time, some cherished creed, some accepted dogma, is proved a heresy, and Bennet's cervical theory fared but little better. A wholesome reaction set in. As experience grew, it was found that pelvic inflammations and strictures of the cervical canal came from this treatment. This discovery led to the abandonment of the more heroic caustics. Then, again, it gradually dawned upon truth-seekers that, far more than inflammation, passive congestion of the womb and of its annexes, together with resulting structural changes, that uterine displacements and injuries, and that ovarian and tubal lesions, played important *rôles* in the female economy. In keeping with this knowledge is the far more rational and beneficial treatment of the present day.

Still, granting that the treatment of actual, visible, and tangible uterine disease leaves little for improvement, the whole truth has not been reached—for truth evolves slowly, and does not, like Aaron's rod, bud and blossom in a night. The ball-and-chain of tradition still drags at the heel of this branch of science, and the medical mind, in close touch here with the lay mind, tends to give the reproductive organs undue importance—to attribute, in fact, altogether too much to their influence. By a very large number of practitioners, even by specialists working in other fields of medicine, these

organs are too often wrongfully made the scape-goat for headaches and nape-aches, for spine-aches and backaches, for weakness of vision, for aural disturbances, for sore-throat and weak lungs, for irritable heart, and also for a host of so-called uterine symptoms. Yet these very symptoms may be due wholly to nerve exhaustion, or malnutrition of nerve centres, and not to reflex action, or to direct action, from some real or from some supposed uterine disorder. I say this advisedly, because I too have thus erred, and because hardly a day passes without my seeing cases of supposed uterine disease which have been so treated for months—even for years—when the whole trouble, or the most of it, lies—not in the reproductive organs, but —in the nerve ganglia. This abuse of uterine treatment through a mistaken diagnosis is, in my opinion, the great medical error of the day.

Once I was asked by a medical friend to see an exceedingly bad and acute case of pruritus vulvæ. Very naturally attributing it to uterine disease, he discovered a small cervical tear, and I was called to decide the question of repairing it. I too was, at first, led astray, but the suddenness of the seizure and the lack of consistency in the behavior of the symptoms, put me on my guard, and we soon found out the cause to be an acute attack of jealousy.

To this day a lady rallies me upon a wild-goose chase which I made after a uterine cause for her neurasthenic symptoms. Carefully overhauling her pelvic organs I discovered nothing but a torn perineum, and attributing to that lesion all her ills, which were many, I restored it, making at the same time the rash but honest promise of her complete restoration to health. The rent was a bad one and needed repair, but it had existed for years without doing harm, and it had nothing to do with her symptoms, although they were largely uterine in expression, notably a bearing-down feeling. They became, in fact, worse after the operation than before, and it was only after recognizing the nerve element of her disease, that I was enabled to do her any good. Many such mistakes, of attributing too much to the reproductive apparatus, have I seen in the practice of other physicians; but my own blunders in the same direction have made me feel very charitably toward them. Out of many examples let me give a few:

Not many years ago I was asked by one of our best and most conscientious physicians, himself a gynecologist and a medical teacher, to see a very delicate and refined lady. She had passed through most of the trials which beset a clergyman's wife, and had borne two children, the younger one ten years before. Having for many years enjoyed fairly good health, she lately had suffered a great bereavement, and she was now prostrated by nerve-exhaustion. Her symptoms were markedly of a uterine character. These were pain in the back, an irritable bladder, a pelvic bearing-down, an inability to walk, aches in the left groin, menorrhagia and leucorrhœa. These uterine symptoms had led my friend astray, and finding an insignificant cervical tear, he attributed to it all his patient's ill health, and, against my advice, operated upon it. The wound healed perfectly, excepting in one suture track on one side of the cervix, where a small fistula remained. His patient becoming none the better, but rather the worse, he laid the blame on this fistula, and tried to close it. Not succeeding, he again called me in. Now, this small fistula had no more to do with the lady's general ill-health than the earring holes in her ears, and her disease was clearly a neurosis. Yet I could not bring my friend to my way of thinking —the uterine symptoms were too much for him—and he tinkered away at the fistula until it finally healed up, but the lady became no better. Finally, he adopted my theory of neurosis, put his patient upon an appropriate treatment, cured her, and then had the manliness to thank me for my advice. My friend is now, alas! no more, else I would not have told this story; and I have done so simply to show how hard it is for a bright and progressive mind, even for a gynecologist and a medical teacher at that, to free himself from the bias of tradition.

By breaking down the nervous system the brain-cramming, the intellectual rivalry, the buckram proprieties, and the unwholesome confinement of our boarding-schools breed a host of sickly girls, who swarm in every class of society. Manifold diseases, both functional and structural, date from the recitation room. They are mostly of a uterine complexion, for at that time of life the sexual sphere dominates, and the brunt of the nervous and of the vascular disturbances which form the essence of nerve-exhaustion, falls on the most exact-

ing organs—the reproductive. Hence these suffer from neuralgic pains or from congestion and the lesions coming from prolonged congestion. Yet physicians, misled by the urgency and the number of the so-called uterine symptoms, mistake the effect, or more often the counterfeited effect, for the cause, and give a local treatment when it should be a constitutional one.

So common is it for girls in boarding-schools to suffer from amenorrhœa or from irregular menstruation, as to create a general impression in the community that in these schools some drug is secretly given in the food in order to lessen the laundry work. In one school of great repute so many girls missed their monthly periods that the family physician of several of them wrote to me, asking whether it were possible, as his patients averred, "that as their clothes were laundried in the building, something was given in their food or their drink to produce the effect, for the purpose of saving the laundress the disagreeable task of washing the napkins." My reply was, that if laundresses were acquainted with a drug that could arrest menstruation, they knew more than the profession; and that his patients had lost their monthly periods, not from secret drugs, but from loss of health due to an unwholesome system of education.

Another physician, the worthy chairman of a school-board in a young city, became awakened to the fact that a large proportion of the girls in the public schools were failing in health from backache, wakefulness, weariness, and disordered menstruation. Full of philanthropic and architectural zeal, and with the uterine idea firmly implanted in his head, he wrote to me, asking whether the long flights of stairs in the school-houses were not to blame for these female ailments. I replied in words to the effect that were a higher culture literal, being perched upon the top of some Eiffel's tower, like Simeon Stylites upon his pillar, and were girls obliged to climb up to it, the muscular effort would no doubt put them out of breath, and repeated intellectual excursions in the same direction might in the end cause—not uterine diseases, but—the heart diseases of athletes and of Alpine climbers. I further contended that stair-climbing *per se* could rarely produce uterine disorders, and that the girls in his school were suffering—not from uterine disease, but—from the nerve-counterfeits of uterine disease, viz., from the reflexes

of a nerve-exhaustion resulting from an unhealthy, and therefore faulty, system of education. My arguments evidently failed to convince this honest gentleman, for the correspondence abruptly ended at this point, and I have a shrewd suspicion that he carried out his architectural plans. Should any of my readers journey westward, and see in some growing city a girls' school-house fashioned like a covered rope-walk or a skating-rink, depend upon it, it will turn out to be the philanthropic but mistaken evolution of my correspondent. The traveller would no doubt find education very effectively taught on the only floor—the ground floor—of this building; but doubtless, also, he would see as many pale faces, and would discover on inquiry no fewer backaches, spine-aches, womb-aches, and menstrual disturbances, than when the recitation rooms were on the third or the fourth floor.

In a parous neurasthenic woman a leucorrhœa, a slight prolapse of the womb, a small tear of the cervix, or an insignificant rent of the perineum, each plays the part of the will-o'-the-wisp to allure the physician away from the bottom factor. To these trifling lesions—because they are visible, palpable, and ponderable, and because he has, by education and by tradition, a uterine bias—he attributes all his patient's troubles; whereas a greater and a subtler force, the invisible, impalpable, and imponderable nervous system, may be the sole delinquent. She may be a bereaved mother, a grieving widow, or a neglected wife, and all her uterine symptoms, yes, every one of them, may be the outcome of her sorrows, and not of her local lesions.

Often the victim of this misdirected treatment is a young unmarried girl, whose nerves have been upset by some secret grief, perhaps a cross in love, or by ambitious over-study. Misleading symptoms now set in, which I cannot better describe than by a quotation from one of my own articles on the subject (*Lessons in Gynecology*, third edition, p. 523):

"She loses her appetite, lies awake at night, and grows pale and weak. She has cold feet and blue finger-nails, and perhaps complains of infra-mammary and ovarian pains. Headache and backache, spine-ache, and an oppressive sense of exhaustion distress her. Her monthly periods, hitherto without suffering, now begin to annoy her more and more, until they become

extremely painful, and at these times dark circles appear under her eyes. Her linen is stained by a leucorrhœa, and bladder troubles soon set in. She is wearied beyond measure by the slightest mental or physical exertion; the short visit of a friend upsets her for the rest of the day; 'a grasshopper is a burden' to her, and she finally becomes very nervous or hysterical. Now, very unfortunately, the idea attached to this group of symptoms is that the womb is at fault. A moral rape is, therefore, committed by a digital or a speculum examination, and two supposed lesions will be found—firstly, as a matter of course, the natural virginal anteflexion; and, secondly, a slight uterine catarrh. These are at once seized upon as the prime factors, and she is accordingly subjected to a painful, an unnerving, and a humiliating treatment for the false flexion and the sham endometritis. Unimproved, she drags herself from one consulting room to another, until finally, in despair, she settles down to a sofa in a darkened room, and becomes either the spoiled pet or the vampire of the family."

That this is not an over-wrought picture, every one of my readers will attest. For what physician has not had the original as a patient, and cursed his luck for the possession?

At the present time there are in my private hospital seven ladies who are rapidly recovering from general nerve-exhaustion and from its sham uterine symptoms. They will get well without any local treatment whatever. Yet all were pronounced by their physicians to be cases of uterine disease, and had been so treated for months, and even for years, without benefit. One of them was urged by an excellent authority to submit to the removal of her ovaries; another, to have her slightly torn cervix sewn up; a third, to have a trifling rent in the perineum repaired. Whilst the fourth, a young girl whose nerves had given way from hard study, had a perfectly healthy and a perfectly poised womb propped up by an anteflexion pessary, treated every other day, for weeks, by an application, and deluged twice a day with gallons of hot water.

Many of us have laughed at the quaint but kind doctor in one of Bulwer's novels, who prescribes a special pellet for each emotion, and calms a passion by its appropriate parvule. But are we, the physicians of the nineteenth century, a century of infinite progress, are we, I say, a whit the wiser? We who seek to cure the nerve-reflexes of grief, of love, of fret, and of jealousy, not by harmless infinitesimals, but by the removal of the ovaries, and by operations upon the cervix and the perineum; or try to salve brain-wounds

and heart-pangs by anteflexion pessaries, by uterine applications, and by vaginal douches of a temperature of 110°? Can the rankest materialism be pushed further than this?

Fortunately for the reputation of my professional brothers, and fortunately for my own,—for I, the heavy Mentor of this homily, have also sinned and in like manner,—this grave error of diagnosis is not without excuse. The symptoms of nerve-prostration so greatly resemble those of even coarse uterine lesions that the nerve-mimicries can very readily be mistaken for signals of actual organic disease. Nor, indeed, are they always distinguishable the one from the other, for the marvellous kinship between mind and matter is a tangled skein, not yet unravelled by dead-house or by laboratory.

What, then, are these symptoms? Their name is legion, but the most common ones are, strangely enough, those which lay and professional tradition, with singular consent, have labelled as the symptoms *par excellence* of womb disease. They are, in the order of their frequency, great weariness; more or less of nervousness and of wakefulness; inability to walk any distance and a bearing-down feeling; headache, nape-ache and backache; cold feet; an irritable bladder; spinal tenderness, and pain in one ovary, usually the left, or in both ovaries. The sense of exhaustion is a remarkable one: the woman is always tired; she passes the day tired, she goes to bed tired, and she wakes up tired—often, indeed, more tired than when she fell asleep. She sighs a great deal, and her arms and legs tremble or "fall asleep" so frequently that she fears palsy or paralysis.

Other symptoms not quite so common are the cerebral ones; such as low spirits, bad dreams, nightmares and night-terrors, explosive sounds in the head, a loss of memory, suicidal thoughts, the fear of impending insanity, the dread of being left alone or of being in a crowd. Some patients are unable to protrude the tongue, or they may have weakness of vision, or a morbid keenness of smell or of taste. Others are kept away from church on account of spinal thrills and locking of the jaws whenever they hear the lower or the tremulous notes of the organ.

The uterine reflexes are: uterine, pelvic, and ovarian aches; pendulum pains swinging from one groin to the other; jerking muscles

which cannot keep still; a trembling or a quivering in the abdomen, or a feeling that it needs support, which is often given by pressure with the hands. The intestines display their interest in the general neurosis by flatulence, by noisy borborygmi, and by the belching of wind in large volumes. They usually keep bound, but sometimes they are loose, or the two conditions alternate. One very common symptom is great exhaustion after a movement of the bowels, and these movements are often caused at unseasonable times by any simple excitement, such as the ring of the door-bell or the call of a friend.

Dyspeptic symptoms are rarely absent, sometimes they are very marked, while the tongue looks perfectly clean, even when nausea, vomiting, or diarrhœa is present.

Some nervous women are annoyed by clammy hands and feet, or by profuse sweats, which often are not general, but either local or unilateral. Thus I have seen one-half of the face always wet, while the other half kept dry. One of my patients perspired so profusely from the tips of her fingers that they were constantly dripping and she could not wear gloves. Once I saw a hysterical nose distilling a clear serum, which for weeks dribbled away almost in a stream. The annoyance was great, yet no treatment whatever did the slightest good; but one day it vanished as mysteriously as it came. Many nervous women are frightened by muscular cramps, or by tinglings, by loss of sensation, or by numbness in the extremities, which are deemed the precursors of paralysis. Nor does the skin escape the general sympathy. It becomes dry, harsh and scurfy, and pigmentary deposits appear under the eyes, around the nipples, and on the chin and forehead. Blondes are likely to get a mottled complexion, and brunettes may become disfigured by brown patches. Sometimes the whole complexion changes to a darker hue. In one instance this was so marked, that both the family physician and the consultant diagnosticated a case of nerve-bronzing to be one of Addison's disease.

Another very remarkable, and often very misleading, symptom is a sham aneurismal pulsation of the aorta in the epigastrium. Repeatedly have patients been sent to me with the diagnosis of aortic aneurism, when their sole disease was nerve-exhaustion.

Lastly, abnormalities in the sexual feeling are not wanting. In the majority all desire is quenched, or intercourse is painful; in some the sexual feeling is increased; in yet others, it is urgent, but it cannot be gratified; in a few, the dreams are erotic.

From this wealth, from this positive exuberance of symptoms, one would suppose that the diagnosis could not offer any difficulty whatever; but this is far from being the case. The laws governing woman's complex organism cannot be codified; nor can we correctly label the lesions of cerebration proper or of innervation in general. Then the instinct of causality in man leads him to forget that things seen may come from things unseen, and to attribute preferably to the visible and to the touchable like phenomena which may come from the invisible and the untouchable. Naturally, then, we are likely to be misled: Firstly, by symptoms which by common consent are deemed peculiar to uterine disease; secondly, by the coexistence of actual uterine disease, to which we attach undue and overshadowing importance. Apart from these reasons there is yet another one: the human mind, bewildered by a multiplicity of details, does not catch the meaning of their aggregate. So Goethe's traveller missed the forest by reason of the wilderness of trees around him, and the hero of our own national caricature "could not see the town, there were so many houses."

From a large experience I humbly offer to the reader the following watch-words as broad helps to diagnosis. In the first place, always bear in mind what another has pithily said, that "woman has some organs outside of the pelvis." *Secondly.* Each neurotic case will usually have a tale of fret or grief, of cark and care, of wear and tear. *Thirdly.* Scant or delayed or suppressed menstruation is far more frequently the result of nerve-exhaustion than of uterine disease. *Fourthly.* Groin-aches and sore ovaries are far more commonly symptoms of nerve-exhaustion than of diseased appendages. *Fifthly.* Anteflexion *per se* is not a pathological condition. It is so when associated with sterility or with painful menstruation, and only then does it need treatment. *Sixthly.* An irritable bladder is more often a nerve symptom than a uterine one. *Seventhly.* In a large number of cases of supposed or of actual uterine disease which display marked gastric disturbance, if the

tongue be clean, the essential disease will be found to be neurotic, and it must be treated so. *Eighthly.* Almost every supposed uterine case, characterized by excess of sensibility and by scantness of will-power, is essentially a neurosis. *Ninthly.* In the vast majority of cases in which the woman takes to her bed and stays there indefinitely, from some supposed uterine lesion, she is bed-ridden from her brain and not from her womb. I will go further, and assert that this will be the rule, even when the womb itself is displaced, or it is disordered by a disease or by a lesion that is not in itself exacting or dangerous to life. *Finally.* Uterine symptoms are not *always* present in cases of uterine disease. Nor when present, and even urgent, do they *necessarily* come from uterine disease, for they may be merely nerve-counterfeits of it.

DISCUSSION.

Dr. Frank Woodbury: I wish to thank Dr. Goodell personally for this paper. The only contribution to medical literature that can be placed beside it are Clifford Allbutt's well-known Gulstonian lectures on "The Visceral Neuroses," in which very similar ground is taken.

I wish in this connection to report briefly the case of a girl who enjoyed robust health to the age of eleven years, and was considered unusually bright for her age. She was then attending school, and was much interested in her studies, and, being in advance of her class, was stimulated by competition to over-study. Her health began to fail, and she had occasional attacks of epilepsy. It became necessary to remove her from school. She afterward developed physically, but her nervous system did not develop proportionately. The epileptic attacks continued, and when she came under my observation she was eighteen years of age, and in her actions resembled a girl of fourteen or fifteen. I examined her carefully, and every avenue studied presented something to be treated. In the upper air-passages I found hypertrophy and obstruction, which I removed. She had chronic indigestion, which I removed by treatment. There was trouble with the eyes, and I sent her to a friend, who, after considerable trouble, succeeded in getting proper spectacles. Her ears required treatment, one membrane being perforated, and the teeth needed attention, and were duly attended to. Finally, after studying every other part, I thought it advisable that the condition of the reproductive organs should be ascertained. There was some trouble with menstruation. I sent her to a friend to be examined, and, on her own

responsibility, she went to some one else. A few days later I received from the latter gentleman, who was a stranger to me, a note stating that he had found pus tubes and diseased ovaries, and that he proposed to operate on her the following Sunday, and would be glad to have me present. I immediately wrote to the father of the girl, requesting him to call, and asked him if he knew anything of the proposed operation. He only knew that some operation was to be done. I told him that I was not satisfied as to the necessity for such operation, and advised against it, and recommended a sea voyage. The girl was sent back to me, although very indignant that I had interfered. with the proposed operation, upon which her mind was set. I found that I had lost the confidence of the patient, and after treating her for a month longer, I again sent her to the gynecologist to whom I had originally directed her. From the history of the case, and the signs of local disease which he found, he thought that an operation would probably be of benefit.

The operation was done, and I can state that there was no pus tube present. The ovaries appeared to be healthy, although one of them was cystic. They were not much enlarged, and did not appear to be adherent. The patient, I am sorry to say, perished at the end of a week, so that the ultimate effect of the operation cannot be stated. I very much doubt that any benefit would have followed.

I have now a patient in the third month of the rest treatment, who came to me from the interior of the State, completely preoccupied with the idea of having the uterus and ovaries removed, as the source of her manifold miseries. She is a typical neurotic patient. She is now, as I said, in the third month of the rest cure; is steadily increasing in flesh and strength; sleeps well, and utterly ignores the idea of operative interference.

Cases like these make us feel the necessity for the wide publication of such articles as the one with which Dr. Goodell has favored us to-night.

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THE

DIAGNOSIS AND TREATMENT

OF

EXTRA-UTERINE PREGNANCY.

BY

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FOTHERGILLIAN GOLD MEDALLIST OF THE LONDON MEDICAL SOCIETY, 1886, FOR ESSAY ON "TYPHOID FEVER," MEMBER ULSTER MEDICAL SOCIETY AND BRITISH MEDICAL ASSOCIATION; LATE ASSISTANT PHYSICIAN COUNTY TYRONE FEVER HOSPITAL; PHYSICIAN JACKSON ALMSHOUSE, AUGHNACLOY, AND MEDICAL OFFICER BELFAST POOR-LAW DISPENSARY, FORMERLY RESIDENT SURGEON BELFAST UNION INFIRMARY; AND RESIDENT PHYSICIAN BELFAST UNION FEVER HOSPITAL; MEDICAL REF. LIVERPOOL INSURANCE SOCIETY.

THE JENKS PRIZE ESSAY

OF THE

COLLEGE OF PHYSICIANS OF PHILADELPHIA.

DIAGNOSIS AND TREATMENT

OF

EXTRA-UTERINE PREGNANCY.

PART I.

DIAGNOSIS.

In order to discuss the diagnosis of extra-uterine pregnancy it is essential to glance at the history and pathology of the condition, as an entire revolution has taken place within the last few years in pathology and treatment.

Until quite recently great ignorance and confusion prevailed as to the causation, varieties, and treatment, so much so, indeed, that the value of the older works on the subject is very small. In particular the value of the older tables of statistics and collections of cases, where those which have little or nothing in common are huddled together, amounts to almost nothing, except for some particular uses; and the deductions from such statistics are often most misleading. The recent enormous strides of abdominal surgery first revolutionized the treatment, and now post-mortem examinations greatly helped by ante-mortem sections have afforded new light on the pathology, which makes clear much which was previously very dark indeed. Lawson Tait has, perhaps, more experience of such cases than any man living, and more than any man who ever lived. The writer is inclined to agree with most of Tait's views upon pathology, although Tait is evidently too dogmatic on several points in diagnosis and treatment. Or, perhaps I should say, he looks on the treatment too much from the highly skillful abdominal surgeon's point of view, and does not consider what would be the most proper treatment to be followed where

such special skill was not available. With regard to pathology the weight of evidence seems to be in favor of most of his views, and now, since attention has been so much directed to the subject, proofs are coming in daily of the truth of positions which he took up years ago purely upon brilliant inductive reasoning. His theories, certainly harmonize with all that has been published on the subject, which no other theories will do. Most apparent contradictions of his views can be quite easily explained, and a great many of them can easily be made to convict their authors of error, out of their own mouths. As to old authors, their descriptions of even supposed facts require to be received with many grains of salt. Indeed, we might commence this chapter with the words of a famous author on this subject: "Special treatises on obstetrics, as well as periodical medical literature, teem with statements which are utterly unreliable, and which are calculated to mislead investigators of this subject." [1]

The work of Parry, and that of Dr. William Campbell, of Edinburgh, published in 1842, contain about all that was known of the subject up till 1878, when it began to develop rapidly through Tait's wonderful success in abdominal section for rupture of extra-uterine gestation cysts during the first four months. One great change in our views, admitted on all hands, is that such cases are much more frequent than was formerly thought. In the light of recent experience, we can see quite plainly that formerly such cases were, as a rule, not only not treated, but not even diagnosed. Such cases were put down to "peritonitis" or to simple hæmatocele, extra- or intra-peritoneal, and the woman was simply allowed to die with whatever comfort could be derived from opium, hot fomentations, hot-water jars, stimulants, etc. The diagnosis of hæmatocele was right enough as far as it went, for the rupture of an extra-uterine gestation cyst is the most common cause of pelvic hæmatocele, of both varieties.

Without further preface or reference to the older literature of the subject I shall proceed to give Tait's classification of the forms which extra-uterine pregnancy may assume, either from the first, or after what Tait calls "primary" rupture. His amended table as given in the *Lancet*, September 1st, 1888, p. 409, is as follows:—

"Scheme of Ectopic Gestations in Tubo-ovarian Tract.

"1. Ovarian: possible, but not yet proved.

[1] John S. Parry, "Extra-uterine Pregnancy, its Causes, Species, etc." Philadelphia, 1876.

"2. Tubal, in free part of tube; and is—

"(*a*) Contained in tube up to fourteenth week, at or before which time *primary* rupture occurs, and then the process of gestation is directed into—

"(*b*) Abdominal or intra-peritoneal gestation, uniformly fatal unless relieved by abdominal section, primarily by hemorrhage, secondarily by suppuration of the ruptured sac and peritonitis.

"(*c*). Broad ligament or extra-peritoneal gestation.

"(*d*) May develop in broad ligament to full time, and be removed at viable period as a living child.

"(*e*) May die and be absorbed as extra-peritoneal hæmatocele.

"(*f*) May die, and the suppurating ovum may be discharged at or near the umbilicus, or through the bladder, vagina, or intestinal tract.

"(*g*) May remain quiescent, as a lithopædion.

"(*h*) *May become abdominal* or intra-peritoneal gestation by *secondary* rupture.

"3. Tubo-uterine or interstitial is (*a*) contained in the part of the tube embraced by uterine tissue, and, so far as is known, is uniformly fatal by intra-peritoneal rupture (as *b*) before the fifth month."

Regarding ovarian gestation, Tait is not alone in the opinion that it never occurs. Many writers of eminence deny the possibility of its occurrence, as Velpeau, Arthur Farre, Mayor, and, more recently, A. Willigk, who minutely examined several supposed specimens, but failed to find fœtus or membranes in the ovary. Here we come to Tait's theory as to impregnation. He believes that, save in abnormal conditions of the tubes, spermatozoa never penetrate further than the uterus. The belief in ovarian gestation arose at the end of last century, from the theory that spermatozoa normally penetrated to the ovary. Hausmann made some researches in animals which were supposed to settle the matter, and the belief got up that most extra-uterine pregnancies were ovarian. This, at least, is self-evidently false at the present day. But Hausmann's conclusions are inconsistent with each other, and the lower animals cannot be said to have Fallopian tubes, as the horns of the uterus extend to the ovary. Campbell has collected a number of cases which might be looked on as genuine, only that they are so old (1682, 1697, 1735, and 1767). Tait alleges there are no preparations in museums, and that no modern preparation will bear examination. The objection to the old ones is that men were in those

days so incapable of minute examination and so prejudiced by their theories that their testimony is not worth much. At present we could only accept the evidence procured by post-mortem examination, or ante-mortem section, in which the examination had been made by a skilled pathologist, and not by an ordinary practitioner who would probably see whatever his theories inclined him to see; or, if not, who would use language too loosely for our purpose. Tait has seen a total of seventy-six cases, all of which were tubal except one, which was interstitial. Tait, in his work just published, disposes satisfactorily of all alleged cases, such as that of Spiegelberg, that of Puech, and those of Hildebrandt, of Berlin ("Lectures on Ectopic Pregnancy," by Lawson Tait, 1888, pp. 8–13).

Professor Arthur Willigk (*Vierteljahrbuch, für Pract. Heilkunde*, 1859) disposes of a number of specimens in various museums which had been labeled "ovarian," including one of Kiwisch. He depends on microscopic examination. On the other side we have men equally eminent, as Coste, Hecker, Kiwisch, and recently Puech (*Annal. de Gynéc.*, July, 1878); also Duverney, Goupil, and P. U. Walter give cases which it is difficult to reject.

In an essay on diagnosis and treatment it would be out of place to enter further into the question, but I may sum up with Parry, who says the weight of authority is in favor of the *probability* of ovarian pregnancy. While denying the probability of quite recent reported cases, one described in *British Medical Journal*, Vol. 1, 1887, p. 568, is a good example, where a Dr. Cargill, of Jamaica, says he found the placenta of a five months' fœtus, embedded in a ruptured cystic ovary. This would seem conclusive, did we not know that in extra-uterine cases the placenta can migrate, change its ground, spread here and there from its original attachment, or may even be detached altogether and take root again (Dr. James Braithwaite, of Leeds, *British Medical Journal*, January 3, 1885).

Professor Freund (*Edinburgh Medical Journal*, September, 1883, p. 243; November, p. 397; December, p. 521), in a record and classification of fifteen cases of extra-uterine pregnancy, recognizes the ovarian variety, and says it is characterized clinically by "little or no pain in early pregnancy; rapidly growing flat tumor at side of uterus; may go on to full time." But in his description of the appearances at operation, he seems to indicate rather the broad ligament form of Tait.

With regard to the tubal, Tait believes that every ectopic gestation is originally located in the tube. He accepts the ovario-tubal as a possible variety which must always become intra-peritoneal on primary rupture, which would always occur within the time which limits the purely tubal form, that is, thirteen to fourteen weeks. All are agreed that the great majority of cases are tubal at first. There is no purpose in making further subdivisions of the tubal cases, as it is admitted on all hands that it is impossible to diagnose one form from the other, except at section or post-mortem examination. The important point here is Tait's theory, that according to the implantation of the placenta in the tube is the direction of the rupture, which *always* takes place within fourteen weeks. That if the rupture happen in the free part of the circumference of the tube, the bleeding is into the peritoneum (intra-peritoneal hæmatocele), which is almost invariably fatal unless the abdomen be opened; but if the rupture should happen in that fourth of the circumference of the Fallopian tube which corresponds to the folds of peritoneum constituting the broad ligament, then the fœtus and blood escape into the substance of the broad ligament. In the latter case the bleeding is restrained by the compressive power of the adhering folds of the ligament, so may not be fatal, and the fœtus may go on developing till term (extra-peritoneal hæmatocele). All the cases in Tait's unique experience (seventy-six cases) in which gestation has gone on beyond *primary* rupture have been of this extra-peritoneal kind. This is a most important advance in the pathology of extra-uterine pregnancy, as although an extra-peritoneal form was recognized by Dezeimeris, who called it "sous-peritoneo-pelvic," yet it was looked on as rare, or denied altogether. However, Drs. D. Berry Hart and J. T. Carter, in a paper read before the Obstetrical Society of Edinburgh, in July, 1887 (*Edinburgh Med. Jour.*, October, 1887), confirm Tait's theory from frozen sections of two cases. They also confirm what he taught for years, that in these intra-ligamentous cases the peritoneum is stripped off the back of the uterus, part of the front, sides and back of the pelvis and abdominal walls round to the front on each side, as far as the horns of the uterus. So that the cyst may present close to the abdominal muscles in the abdomen, and, although the placenta may be found extensively attached, for instance, to the back of the uterus, sides of pelvis, abdominal wall, and even to the bladder and intestines, still, if *secondary* rupture of the broad

ligament has not occurred the whole fruit of the gestation may be quite extra-peritoneal, and may even be removed by laparotomy without touching the peritoneum. So as Tait says, some of his operations are true abdominal sections, some are not. Hart and Carter propose a new name for the cases in which the peritoneum is stripped off the anterior abdominal wall—"sub-peritoneo abdominal" gestation. They also deny that either ovarian or purely intra-peritoneal abdominal pregnancy has ever been accurately proved to exist. Tait does not believe in the occurrence of primary abdominal pregnancy (intra-peritoneal). He thinks the digestive power of the peritoneum would dispose of the freshly impregnated ovum which dropped into it, in the same way as it does normally with the non-impregnated ova, which he believes are always dropping in.

The interstitial variety must be extremely rare; as far as known they always rupture into the peritoneum. The only other route they could take would be into the cavity of the uterus, when the case would be indistinguishable from normal pregnancy. Such statements merit no serious attention, as it is quite absurd to talk of diagnosing an interstitial case from a normal pregnancy. Tait has never seen a preparation which could, by any possibility, have been diagnosed from normal pregnancy during life, or, at least, previous to rupture. The time of rupture varies from three to twenty weeks.

My reason for accepting Tait's opinions, as a rule, in preference to those of others is, that he gives nothing as fact which has not been verified either by ante- or post-mortem section, or museum specimens. Against opinions thus established we have those of men who argue from impressions derived from symptoms, or pelvic examination, or deductions from lists of cases unearthed here and there, imperfectly observed and recorded, or records of post-mortem examinations made by men unskilled in anatomy. The acceptance of doubtful evidence has been the principal reason of the slow advance of medicine as a science. Imperfect observation, false experience, and the records of false diagnosis, lead to all the absurd and fanciful theories which have been the shame of medicine at all times. Then, authors laboriously collect hundreds of worthless records of cases which, probably, had little in common, and draw conclusions, and lay down rates of mortality and rules of practice for this and that condition; the whole thing being far more false and useless than is the Registrar-General's

Records of the causes of death in Great Britain. Having been a public Registrar of Deaths, I can testify that in most cases the medical certificate gives only the faintest idea of the seat of disease—not taking account of the many cases where the diagnosis is entirely at fault. Yet on these figures theories are being built up! The personal experience of highly qualified and highly gifted men, who systematically record and analyze their cases, is infinitely superior, as a basis for argument, to any form of collective investigation which has hitherto appeared.

I shall say no more regarding the pathology of extra-uterine pregnancy, except incidentally as it may bear directly on diagnosis or treatment. My justification for saying so much is that it is necessary to explain my position on this question, and to explain the terms which I shall have occasion to use. Unless we have an idea of what a man believes as to the nature of a condition, or disease, we can hardly comprehend what he says as to its diagnosis and treatment. I shall now consider the subject of diagnosis under two heads, viz: before rupture and after; the latter being subdivided into several heads.

DIAGNOSIS OF EXTRA-UTERINE PREGNANCY PREVIOUS TO RUPTURE.

Is such a thing possible? Tait practically says no, or that we shall fail so often that it amounts to the same thing. But in the literature of the subject, of recent date, there are many cases recorded where the diagnosis was made and verified either by rupture occurring later on, by operation, or by post-mortem examination. I do not argue from cases where cure resulted from some mode of treatment not involving opening of the abdomen, although it would hardly seem sound judgment to reject all these.

There are two elements in the diagnosis: First, we must diagnose pregnancy, and second we must ascertain with certainty that the pregnancy is extra-uterine. We may at once grant that, practically, in the great majority of cases the thing is impossible, but principally for three reasons: First, that the patients rarely apply for advice until rupture has occurred, that is, we rarely get an opportunity; second, the menses go on, and so the theory of pregnancy is apt to be dismissed; third, the whole history is apt to be misleading. The bulk of the patients have no suspicion of anything wrong till suddenly and without warning rupture occurs.

Well, then, when opportunity for a diagnosis offers, we have first to establish the fact of pregnancy, and this we know it is impossible to do with certainty till the middle of the fourth month, or beginning of the fifth, when the fœtal heart becomes audible. Depaul believes he has heard the fœtal heart as early as the eleventh week, and Routh believes the heart can be heard by vaginal stethoscopy at this early period. Naegle never heard the heart before the eighteenth week, more generally at the end of the twentieth. But for practical purposes pregnancy must have reached the fifth month before we can be sure of hearing the heart. Tubal rupture, however, always occurs before the fourteenth week, usually before the twelfth, and may be as early as the seventh or even fourth week. So that, as a rule, *primary* rupture, usually meaning death or laparotomy, has occurred before we can hear the fœtal heart or be perfectly certain as to whether the patient is pregnant! I need not discuss the other ordinary signs of pregnancy, but will proceed to inquire: Are there any other signs by which we might make a probable diagnosis of pregnancy in the very early stage?

Several new signs of pregnancy have been introduced in recent years, but, except the character of the sphygmographic tracings in pregnancy, which have been studied by J. W. Ballantyne, M. B., Assistant Professor in Midwifery in Edinburgh University, there are none which can assist us much, under the circumstances. The pulse during pregnancy is one of high tension, as shown by the sphygmograph. There is also an increase in the rapidity of the pulse during gestation, the pulse rate usually varying from eighty to eighty-four per minute. The high tension is due to cardiac hypertrophy, as has been shown by the observations of Ducrest, Larcher, and Blot. But, unfortunately, this high tension is best seen in the later months of pregnancy, and in primiparæ. The sign, too, is of more prognostic than diagnostic value. Still, we might obtain a hint from it in obscure cases. The researches of Ballantyne are to be found in *Brit. Med. Jour.*, Vol. 2, 1886, p. 1094. The facts have also been attested by Marey, Mahomed, Macdonald, Fancourt Barnes and Louge. The experiments of Löhlein and others show that the French authors overestimated the amount of cardiac hypertrophy in pregnancy. Then there is Hegar's sign of pregnancy, viz., a characteristic softness, pliability and thinning of the lower segment of the uterus, that is to say, of the

part immediately above the insertion of the sacro-uterine ligaments. This condition can be easily made out, not only when the uterus is of natural firmness, but also when it is flaccid and yielding. In any case, it is possible, by depressing the uterus, to distinguish the upper portion and the rigid cervix from the lower portion, the softness of which is so marked that one could imagine the cervix to be simply in contact with a pelvic or abdominal tumor. No pathological condition known, not even hydrometra or hæmatometra, is capable of producing a similar group of symptoms. But Hegar's sign is not constantly present. (*Prager Med. Wochenschrift*, No. 26, 1884). I do not know of any observations as to the presence of this symptom in extra-uterine pregnancy, but as the uterus develops to a certain extent in this condition, I should think Hegar's sign would be present to some extent. Renil says the sign is present in the third month. Then, Dr. Henry D. Fry (*American Jour. of Obstetrics*, October, 1884) says that between the fifth and the eleventh week of pregnancy he found elevation of the temperature of the vagina, along with the purple color of the mucous membrane. In thirteen cases of non-pregnant women who were examined, the average vaginal temperature was found to be 99.1° F., whilst the average vaginal temperature of the pregnant women was as high as 99.74° F. He concludes that the increased heat is due to active hyperæmia necessary for the growth and development of the ovum and of the uterus. This causes increased intra-uterine temperature, which produces increased vaginal temperature. But, then, pathological conditions of the uterus produce an equal increase of heat in the vagina. Several authorities, as Cohnstein and Fehling, have recognized the intra-uterine rise of temperature during pregnancy. They say the temperature is still higher in the impregnated uterus than in the vagina, and look on that state of things, in the absence of pathological conditions, as indicating not only pregnancy but also a living fœtus. Fry thinks it could be said with more truth that a vaginal temperature equal to or more than 99.7° F. is a strong presumption in favor of impregnation, provided there are no pathological conditions present in the uterus, and no increase of heat in the axilla.

Dr. Routh declared at a debate on the treatment of extra-uterine pregnancy, held at the Brighton Meeting of the Brit. Med. Association, 1886 (*Brit. Med. Jour.*, December 4, 1886, p. 1093), that the uterine

souffle could be heard with the vaginoscope before the third month, though it could not be heard through the abdominal walls, as he has also declared he has heard the fœtal heart.

These are the only observations I have to offer as to recent advances in the diagnosis of pregnancy in general, with the exception of the discovery of Dr. Adolph Rasch (*Brit. Med Jour.*, August 30th, 1873, p. 261), that pregnancy (uterine) could be diagnosed in some cases as early as the seventh week, and in most cases after the second month, by *fluctuation* in some part or corner of the uterus, wherever the ovum happened to be located. Some other fluid retained, as menstrual, is the only possible source of error. He says he has practiced this mode of diagnosis for ten years, and is only confirmed in his opinions. By two fingers in the vagina and the other hand on the abdomen it is possible to detect the smallest amount of liquor amnii just as easily as a few drops of pus can be detected in a subcutaneous abscess. One would, of course, require to be thoroughly accustomed to the bi-manual mode of uterine examination. Dr. Rasch also mentions a symptom not noticed in books which, at least, ought to provoke further inquiry, viz—an increased desire to pass urine, especially at night. This symptom might, of course, be due to any condition causing albuminuria, of which rising to urinate is an early symptom, to diabetes, and to bladder trouble; but then most symptoms are open to many interpretations.

This fluctuation symptom would be absent in extra-uterine pregnancy, but the point of it is that it could be used as a *substitute for the uterine sound* in ascertaining that the uterus was empty, which is a most important point.

After offering these remarks on the subject of the early diagnosis of pregnancy in general, I have to admit it cannot often be done with any reasonable degree of certainty before the fœtal heart becomes audible, before which period rupture usually takes place in extra-uterine pregnancy.

ON THE DIAGNOSIS OF THE EXTRA-UTERINE CHARACTER OF THE PREGNANCY FROM NORMAL GESTATION.

In these cases all the usual sympathetic disturbances of pregnancy are present. Enlargement of the breasts, darkening of the areolæ, increase of Montgomery's tubercles, morning sickness, vomiting, etc.,

are all present. The condition as to menstruation, however, is a most difficult and misleading one. The text-books, still influenced by the opinions of Antoine Petit, who wrote in 1710, usually state that there is an arrest of menstruation, but that after the absence of one or more periods there is often an irregular hemorrhagic discharge, which Barnes attributes to partial detachment of the villi of the chorion, caused by the ovum growing out of proportion to the tube in which it is contained. This is very true as regards many cases, but, unfortunately, we meet with every condition of the menses. Petit says that "the menses, contrary to what is seen in normal gestation, continue to appear, but in smaller quantities, throughout the pregnancy." This is also true of a large number of cases, but, again, not universally. Sometimes the menses are suspended altogether, as in ordinary pregnancy; more commonly they continue irregularly and in *too great quantity*. Sometimes the loss amounts to metrorrhagia, from the enlargement and emptiness of the uterus, which would cause suspicion of a uterine tumor. In fact, the state of the menses is more apt to delude than to help us. The text-books emphasize the point that the patient generally considers herself pregnant, and they reason from this, or rather insinuate, that all the medical man has to do is to locate the pregnancy. Now, this may be sometimes true, but oftener the patient has no suspicion of pregnancy. The basis for the text-book statement is the fact that when the fœtus lives on after the primary rupture the patient comes to know that she is pregnant, and firmly adheres to the idea, should the child be retained for fifty years. But before the period of rupture the case is quite different. Tait, with his enormous experience, finds that the great majority of his patients in whom primary rupture occurred had no suspicion of pregnancy. Consequently, no opportunity for diagnosing extra- from intra-uterine pregnancy was afforded to him. This is the great bar to the diagnosis. Had we cases under observation in hospital, no doubt the extra-uterine character of the pregnancy would be discovered, but from the circumstances, this can seldom be attempted. There is one point that should attract attention and lead to close examination, viz—the fact that the subjects of extra-uterine pregnancy, in a great majority of cases, have either never been pregnant previously, or not for a number of years. At all events, it is the rarest exception in the world, to the general rule, for a woman to make any complaint, or to consult a

doctor, until rupture has actually taken place. Tait has once only been consulted prior to rupture. In this case it was evidently impossible for any man to diagnose the cause of the tubal swelling. He describes it in the *Brit. Gynæcological Journal*, Part xiii, p. 38. She complained of obscure pelvic pain of several months standing, the usual symptoms, in fact, of salpingitis. She had not missed one period. The idea of pregnancy never entered the mind of any medical man, or any other person. Yet, three days afterwards she presented herself with the easily diagnosed symptoms of ruptured tube and abdominal hemorrhage. The history of the case, then, previous to impregnation is usually all we have to guide us. Often one confinement, then an illness with pelvic trouble, then sterility for a long number of years with menstrual pain and distress, then the extra-uterine pregnancy. Parry says, "women who have become pregnant with a child outside the uterine cavity frequently show a previous inaptitude for conception. The interval between marriage and the first impregnation is frequently long. If the woman has borne children a period of sterility frequently precedes the extra-uterine pregnancy." This fact strongly confirms Tait's idea that extra-uterine pregnancy is caused by whatever removes the ciliated epithelium from the Fallopian tubes. The recent discovery of Bland Sutton and Arthur Johnstone further strengthen the view.

If we could arrange the circumstances we could, no doubt, often diagnose either extra- or intra-uterine pregnancy. Given a woman who had incurred the risk of impregnation, who was uneasy about the matter, and who was quite willing to have a minute pelvic examination ma le, we could at a very early period make a highly probable diagnosis of pregnancy in any situation, irrespective of the menses; but the fact is that in real life we rarely or never get the chance. This is proved to demonstration by an extraordinary case of Lawson Tait's, described at page 49 of his work on Ectopic Pregnancy. This woman, if any one in the world would, was just the one to be uneasy enough to have herself examined, as she had already undergone abdominal section for ruptured tube. Yet with her actual experience of the condition and the dreadful results, when the same thing happened a second time, and *knowing she was pregnant*, she never thought of being examined the second, any more than the first time. In fact, there were no symptoms in the slightest calling for any examination.

The state of the menses during extra-uterine pregnancy may lead us into yet another error. From their profuse nature, where the patient has suspected pregnancy, we may actually fancy abortion has taken place, and thus be less prepared than ever for the coming rupture. Campbell illustrates the axiom "Put not your trust in histories," as follows: "In many instances of the different varieties of misplaced gestation, the catamenia are suspended; frequently, however, they appear regularly in each of the early months; in some cases they flow at uncertain periods; and in other examples they are either profuse, or limited in quantity. In many cases, at an uncertain period of gestation we have hemorrhage, uterine effusions, the extrusion of coagula, of bodies which resemble moles, or portions of placenta. These appearances have occasionally led to the belief that the patient has actually aborted, so that the ovum was originally not extra- but intra-uterine, and had escaped through a rent in the uterus into the peritoneal cavity, the extruded body in either case being viewed as the placenta. Cases attended with much uterine excitement, whether arising from unusual exertion, or some external injury, are the most likely to be accompanied by these latter phenomena." So that in every case of fancied abortion we should be on the lookout for extra-uterine pregnancy, among many other causes of uterine colic and hemorrhage.

We have now lingered long enough on the almost negative though usually considered positive signs of extra-uterine pregnancy in the early period. Let us now look at some of the positive changes which would aid us in the diagnosis—opportunity being given.

It is now universally admitted that the uterus undergoes a certain amount of development and evolution, as in normal pregnancy. The lining mucous membrane also undergoes development into a true decidua. Indeed, it has been suggested by an authority that we should dilate the cervix and ascertain the presence or absence of the decidua as a crucial test as to the existence of extra-uterine pregnancy. The hint might possibly prove useful, as in case it turned out to be a normal pregnancy the worst that could result would be an abortion. The cervix uteri also becomes softened and shortened, just as in normal cases, but these changes do not proceed so far. One feature of the abnormal cases, however, which is worthy of note is that the cervix very soon becomes open and remains so. At about the full period of

gestation Tait found, in most of his cases, that the cervix would admit the finger although it could not be passed through the internal os. Post-mortem the decidua has often been found wanting, but the description given by Campbell of what happens at the pseudo-abortions so common during extra-uterine pregnancy explains the matter. Duguet (*Annales de Gynécologie*, May, 1874) has also pointed out that it is usual for the uterine decidua to be thrown off, either in shreds or *en masse*, during the profuse menses which so commonly occur previous to rupture of the tube. The uterus undergoes involution after the death of the ovum, if the patient survive, unless the placenta be implanted into its posterior surface, which so frequently happens. The development of the uterus and the shortening and softening of the cervix, however, only go on to a certain point. Usually the uterus does not exceed the size of that at four months of a normal gestation, and it may not exceed the size normally reached in two months. Parry especially studied this point : he says, " the uterus, except in some rare instances, undergoes striking alterations, both in its structure and volume. Its development has been found to vary from twice the size of an unimpregnated organ to the volume which it is known to attain when gestation is four months advanced." Virchow, Kiwisch, and Oldham have demonstrated that no true decidua is developed in the tube. The mucous membrane there is wanting in the utricular glands which are found in the lining of the uterus. The chorion villi are directly implanted on the mucous membrane, which they even pierce. They even go deeper, encroaching on and weakening the muscular coat, which is, at places, quite displaced. This has an important practical bearing, as the situation of the placenta determines the site of rupture, and the site of rupture decides the further course of the gestation. If the rupture occur in the floor of the tube we have an extra-peritoneal hæmatocele, an effusion of blood into the folds of the broad ligament. The sac of the ovum may not burst. The limited space, the meshes of the connective tissue, and the resistance to further distention of a distended broad ligament may prevent fatal hemorrhage. The ruptured tube, and the cavity of the broad ligament together, may provide a capsule for the ovum, where, except secondary rupture into the peritoneal cavity occur, with its almost certainly fatal hemorrhage, the ovum may develop to maturity and, if suppuration of the sac do not occur, where the child may remain quiescent for

years, or from which it may be almost completely absorbed. In the Fallopian tube the placenta is developed on a different plan from that of the normal placenta. Hennig says it is developed on the plan of the same structure in dogs, cats, and rabbits. Being thus very slightly attached the tubal placenta is easily separated, and hemorrhage easily occurs. It can also take root again on a new site if displaced. This is the explanation of many supposed "ovarian" pregnancies.

A case which occurred in the Birmingham and Midland Hospital for women, under the care of J. W. Taylor, F.R.C.S., illustrates and enforces many points in the diagnosis and treatment of extra-uterine pregnancy in the earlier stage. The patient was 38 years of age; last child born *twelve years ago.* She was seen on 4th March, 1888; menstruation had been regular until 6th December preceding. On this date she had an irregular loss which continued more or less for two months. This loss had ceased three weeks before she was seen; *no amenorrhœa at any time.* Has had pain in right abdomen with a sense of illness for four months. For some time previous to being seen this pain had been increasing; when seen it was unbearable, and prevented sleep. A month before consultation there was some swelling above; this steadily increased up to that time. So here we had a case where the pain drove the patient to the doctor in the early stage, though not before rupture. What did he make of it? He found a hard, tense, *semifluctuant* mass on the right side of the uterus, pushing the latter to the left. The upper limit was quite plain above the pelvis, and formed a crescentic outline from the crest of the ilium to the pubes. He passed a sound, found the uterus slightly enlarged, empty, and distinct from tumor. Urine yielded half its bulk of albumen. What was the diagnosis? An inflamed and probably suppurating abdominal cyst. In fact, much the same as Lawson Tait made on the only occasion on which he was asked to examine an extra-uterine gestation previous to rupture. In the case before us also, as in Tait's, the fact that there had been no amenorrhœa was held sufficient to *exclude pregnancy.* Now, surely this would not be good practice, suppose it was normal pregnancy only we suspected. But with the well-known uncertainty as to the state of the menses in extra-uterine gestation, what shall we say of this summary exclusion? Would it not be nearer the real truth to guess that the possibility of extra-uterine gestation never crossed the mind of either examiner?

Well, abdominal section was done on March 7th. A blackish cystic mass was seen on the right side, very adherent to the intestine and omentum. The tumor was separated somewhat from its adhesions and at a fluctuant part a small *trocar was inserted*—for what earthly reason is not stated. Pure blood flowed freely. The tumor was then quickly separated, when at last the secret came out as a large fœtus of about four months, with its placenta. The latter was mainly attached to the hypertrophied and expanded interior of the Fallopian tube, which had apparently ruptured much earlier. The tumor was then removed and the remains of the tube ligatured, a piece of omentum to which the placenta adhered was removed, the peritoneum washed out, a drainage tube inserted, and the patient was discharged on March 28th with only a trace of albumen in the urine.[1] This case affords obvious lessons in diagnosis and treatment, besides showing that in pregnancy, at least, severe albuminuria is no bar to operation. But the object of introducing the case here is to show that, supposing the hemorrhage following primary rupture should not be severe, and especially if, as in this case, the rupture occur into the broad ligament, we may not only have no symptoms calling for examination previous to rupture, but rupture itself may fail to attract the patient's attention enough to cause her to seek advice. In this case it was only the pressure symptoms of a rapidly growing mass which caused the patient to consult a medical man. The lessons of the case as to treatment are not in order here.

Now let us contrast what happens in real life with the statements of those gentlemen who consider the diagnosis of extra-uterine pregnancy previous to rupture perfectly easy. Dr. James Aveling, in a paper read at the Gynæcological Society on February 8th, 1888 (*Brit. Med. Jour.*, March 10th, 1888, p. 535), says truly enough that, as a rule, erratic pregnancy is found to occur during a prolonged sterile period following a first confinement; that the history is of the greatest importance and should never be overlooked; *that metrorrhagia is important when occurring with the usual sympathetic signs of pregnancy.* But in face of the instances I have alluded to, what shall we say of the statement that the patient usually believes herself to be pregnant, and that there is something unusual about her condition. Why, if this were so it would suggest the diagnosis to the most care-

[1] *Brit. Med. Journ.*, Dec. 1st, 1888, p. 1217.

less. But the statement seems the direct opposite of what usually obtains. Is this not one of Antoine Petit's ideas, which has survived since 1710, of which Parry says, "Could they be verified the detection of extra-uterine gestation would be an easy task; but unfortunately for the comfort of the obstetric surgeon scarcely one of them contains a grain of truth; yet strange to say the opinions of Petit influenced and impeded the progress of our knowledge on this subject for more than half a century." Tait says Parry might have said for a century and three quarters. In addition to the history, the patient's feelings and notions, all most dangerous as guides, of course, the results of pelvic examination are made the basis of diagnosis. What we want above all things is to have the possibility of extra-uterine pregnancy present to the mind. Without this, the most experienced will almost always go wrong. If we really have the possibility before us, and depend almost entirely on the pelvic examination along with the patient's history previous to impregnation, the probabilities are that we would go right in the very rare opportunities presented. It is the very rarity of the discovery which makes men miss it when the chance presents. Repeated examination at intervals of a week or two should certainly lead to discovery of the truth, at all events. A rounded, elastic, semifluctuant, tender tumor behind and to one side of a slightly enlarged and laterally displaced uterus, if found to be rapidly increasing under circumstances which permit the possibility of extra-uterine pregnancy, could hardly be mistaken for anything else. Dr. Barnes maintains (*Brit. Med. Jour.*, March 10th, 1888, p. 536), that the diagnosis of tubal gestation might be made with reasonable certainty, even at the seventh or eighth week. The ordinary signs of pregnancy being present, there are two signs which call for special attention. These are first, pain more or less acute in the pelvic region, of a spasmodic character; secondly, more or less hemorrhagic discharge from the uterus and vagina. *These are signs of abortion*—it might be intra-uterine—but they call for special pelvic examination. The uterus would be found not, as normally, in the middle line, but pushed across the pelvic brim to one side; on the opposite side would be found an extra-uterine swelling of an ovoid shape. The size of the uterus itself might or might not, most likely would not, equal that of the supposed period of gestation. If all these conditions had developed in a short period in a person hitherto free from pelvic distress, the case for extra-uterine

gestation would amount to more than suspicion. If in a week or two the extra-uterine swelling had increased considerably, and shreds of decidual membrane had come away with the metrorrhagia, the case for tubal gestation would be all but complete. The process of abortion would be fairly commenced. If the abortion were uterine we could well wait, interfering only when necessary. But abortion in a tubal case means bursting of the sac, with commonly a fatal result. Dr. Barnes' views may be at once admitted to be in accordance with truth, experience and common sense, but unfortunately the conditions he lays down are rarely fulfilled, which means that the number of times we shall succeed in the diagnosis will be small compared with the failures. By the ordinary signs of pregnancy, I believe he means the sympathetic signs, as enlargement of the breasts, etc., and not the patient's idea, or the history as to menstruation, which, on the whole, usually lead us astray. Again, too often his special signals for pelvic examination, viz., spasmodic pain, and metrorrhagia, are either absent or so slight as not to cause complaint on the part of the woman. However, if we could fix in our minds the idea of Dr. Barnes, that abortion may be extra- as well as intra-uterine, it would perhaps occasionally save us from error. Dr. Routh (*Brit. Med. Jour.*, March 10th, 1888, p. 536) depends on hearing the uterine souffle by means of the vaginoscope as *early as the sixth week.* We know that a similar sound is heard in other cases, fibroid tumor for example, but Dr. Routh very ingeniously demonstrates how we can distinguish to which condition it is due. He argues, perfectly justly, that a fibroid tumor giving rise to a souffle must be much larger than a three months' pregnancy. Besides, there are other marks of distinction. Uterine tumors, while small, and so liable to be confounded with extra-uterine gestation, are always hard and solid. Their rate of growth is much slower and the history is usually a somewhat lengthened period of metrorrhagia. But if the surgeon resorted to bi-manual pelvic examination with the possibility of extra-uterine gestation and of fibroid of the uterus clearly before his mind, I do not think he could possibly make a mistake—especially if he used the sound. With regard to the use of the sound in the diagnosis of extra-uterine pregnancy, I would not hesitate to use it if really necessary, but I think the bi-manual method of examination has very nearly removed the necessity. With two fingers in the vagina, and the other hand in the pelvis from above, we can

usually ascertain all we want to know about the size, shape, and condition as to emptiness, of the uterus. Then we can employ Rasch's sign (*Brit. Med. Jour.*, vol. ii, 1873, p. 261), viz.,—fluctuation in some corner or part of the uterus, to detect the presence of normal pregnancy as early, sometimes, as the seventh week, and in most cases after the second month. For these reasons I think we should seldom feel called upon to employ the uterine sound in cases where the chances, perhaps, are in favor of the pregnancy being intra-uterine after all. There is generally supposed to be no danger in employing the sound, save that of causing abortion in case the pregnancy prove intra-uterine, but this is not altogether correct. We should remember that the great German obstetrician, Braun, in using the sound for the very purpose under consideration, passed it through the fundus without encountering any resistance to warn him, until the point could be felt quite plainly underneath the skin of the abdomen. That he really did so was verified by post-mortem examination, as we would naturally expect. Then the use of the sound has in too many cases been followed by pelvic cellulitis and septic inflammation, which are both dangerous to life and the cause of great suffering to the patient. What can we ascertain by the sound to compensate for its many dangers? Simply that the uterus is empty, its size, and the fact that it is distinct from the tumor. Now I maintain that all these points can be decided by a thorough bi-manual pelvic examination, in most cases. Possibly in diagnosing retroversion or retroflexion of the gravid uterus from extra-uterine gestation, the sound might be essential; but rarely if the examiner thoroughly understand the principles of bi-manual examination.

To return to the main question, W. T. Lusk, M. D., New York, read a paper on abdominal pregnancy and the desirability of early laparotomy, at the annual meeting of the British Medical Association, at Brighton, 1886, with tables and an analysis of 103 cases (*Brit. Med. Jour.*, December 4th, 1886, p. 1083). He gives the clinical history of a case on which he founds his opinions. This woman was under professional observation of her family doctor and another, all through her pregnancy; through the severe sufferings of the primary rupture and the sufferings after this, from the extra-peritoneal hæmatocele and growing fœtus the result of rupture into the broad ligament. She was under observation even through the false labor, at term, when Dr. Mead satisfied himself that the uterus was empty, as he could so

easily do. After all this, Dr. Mead took her to a very eminent gynæcologist in America, who not only went wrong in the diagnosis, but expressed himself with great positiveness as to the absence of extra-uterine pregnancy, and stated that, without doubt, the tumor was a cysto-fibroma, situated in the posterior uterine wall. It seems a fairly typical case. Barnes' false abortion was markedly present, so far, at least, as pain was concerned. False labor at term was quite characteristic, yet, with a history which one would think, on reading it, could belong to hardly anything else, we find the case never diagnosed till long after fœtal death at term. I think such a case as this should prove discouraging to those who tell us that extra-uterine gestation is not hard to diagnose. Dr. Aveling tells us extra-uterine cases are more easily diagnosed than cases of normal pregnancy, and that at the early period.

We are on the horns of a dilemma; either we must consider the medical attendants in Lusk's case fools, or the statements on the other side false. The only way out of the difficulty seems to be, the conclusion that where everything conspires to aid us the diagnosis is extremely easy, but that practically, so many essentials are, as a rule, absent, that very few cases are really diagnosed till an advanced period. I do not question the diagnosis in the case of Dr. Aveling. This seems to have been one of the fortunate cases where it was quite plain. But he seems to think that is the rule, else his rules of treatment by electric current prior to rupture are unmeaning. For, as Tait says, it would be rather hazardous practice to apply the electric current to every kind of pelvic lump, on the suspicion that it might be an extra-uterine gestation sac.

Dr. Aveling says (*Brit. Med. Jour.*, December 4th, 1886, p. 1091) that the diagnosis is not so difficult as some writers would have us suppose. It has so many prominent and characteristic symptoms *that its detection is more easy than that of ordinary pregnancy in the early months.* He says his patient became pregnant soon after marriage, and then remained sterile for four years. After being quite regular, she ceased to be so, and symptoms of pregnancy appeared. Then she was seized at intervals with the severe characteristic colicky pains in the hypogastrium, and a continuous sanguineous discharge from the uterus. By *repeated* examinations a *pulsating* tumor, gradually increasing in size, was discovered behind and to the left of the uterus,

and this organ became, by degrees, evoluted, until it was at least four times its natural dimensions. Lastly, decidua was expelled from the uterus, making the diagnosis, as he says, of the left Fallopian gestation almost a certainty. This may be looked on as a typical case of the easy kind. It is hard to see how any man would either dispute the diagnosis, or, indeed, come to any other. But they are not all of that sort. In the very next case, described at the same meeting of the British Medical Association (*Brit. Med. Jour.*, December 4th, 1886, p. 1092) by R. Petch, M. D., London, the medical attendant missed the diagnosis, *although it is easier than in normal pregnancy*—misled, again, by the history, as the woman was suckling the previous child and the menses had not returned. Here there was neither a prolonged sterility nor any change whatever in the menses to suggest pregnancy, and accordingly we find it never suggested itself to the doctor. Her health had been good until two years before, when her only child was born; she suckled it for fifteen months, and became very weak and anæmic. The first return of the menses was in February, 1878, when she began to suffer from slight metrorrhagia at irregular intervals; she became very constipated and had pain after movement of the bowels; the characteristic pressure on the rectum described by Tait as "annular stricture," which he looks on as a proof of effusion of blood into the *left* broad ligament. She gradually got weaker and suffered from occasional attacks of "obstruction of the bowels," the pain becoming so severe and constant that she had to spend most of her time on the sofa. In April she first noticed a swelling in the lower part of the body, on the *right;* it was as large as an orange and tender. About the middle of June *she was aspirated by* her doctor for *supposed hæmatocele,* several ounces of blood being withdrawn, with some relief to pain. On July 8th there was found between the umbilicus and Poupart's ligament, on the right, an oval tumor, well-defined, hard, and somewhat irregular on its surface, not movable, tender. No fœtal heart sounds or movements could be detected, but at at a point midway between the umbilicus and the anterior superior spinous process, a bruit resembling the uterine souffle was distinctly heard. On pelvic examination the uterus was found pressed to the left and forward, with the os low down and so patent as to admit the finger for 1½ inches. A bougie passed 4½ inches; there was slight muco-sanguinolent discharge. On July 12th the fœtal heart sounds were very distinctly heard; a few

days after fœtal movements were detected, and were recognized also by the patient. On August 20th treatment by galvano-puncture was commenced. Now, I hold that this case is just as typical of the difficult class of cases as that of Dr. Aveling is of the easy class. As the sac was on the right side the bowel obstruction must have been due to some other cause, probably, than effusion of blood into the left broad ligament, as insisted on by Tait. However, it is not at all mysterious that a child in the pelvis should obstruct the rectum, without regard to position. Now, Dr. Aveling was present at the discussion which followed on these papers, yet we do not find him expressing any surprise that the early diagnosis of this case was missed—nor offering the slightest explanation, or pointing out how it could "easily" have been done. Here we have a case under medical care and unlimited opportunities for examination, go on through the primary rupture and up to the sixth month without being diagnosed, or, at least, not fully diagnosed. The diagnosis of hæmatocele was right enough as far as it went. She was certainly suffering from extra-peritoneal hæmatocele, the result of tubal rupture, but the fact that there was also a living, developing fœtus in the broad ligament was quite overlooked.

Dr. Routh, at the same debate (*Brit. Med. Jour.*, Dec. 4th, 1886, p. 1094), said that four signs made the diagnosis easy in the first class of cases, viz., up to the third month : (1) the general signs of pregnancy, (2) the shifting of the uterus to one side by a growing tumor on the other, (3) stoppage of the catamenia, and, often, subsequent delivery of either the whole or part of a decidua, and (4) detection of uterine souffle with vaginoscope, and not through abdominal walls. Yet he had just heard the cases of Dr. Lusk and Dr. Petch related, where the medical attendants failed to diagnose such cases till a late period. Dr. Hanks, New York, said at the same debate (*ibid.*) that tubal pregnancy ought to be diagnosed and could be diagnosed. A hæmatocele (simple?) was accompanied by excessive pain, and attended with general signs of loss of blood, but with no evidence of pregnancy. He related a case where the diagnosis was made apparently by the discharge of a bit of decidua which a microscopist after examination declared not to be decidua. The post-mortem examination confirmed the diagnosis. Mr. Lawson Tait said that he, with the largest experience yet on record, did not believe that even were opportunities given,

a diagnosis could be made before rupture with any degree of certainty. In fact there were no symptoms in extra-uterine pregnancy before the rupture had taken place. He also said he did not know of any one who had ever asserted that he had made a diagnosis of tubal pregnancy before rupture had taken place. He had never been called in to such a case certainly. Dr. H. A. Kelly, Philadelphia, immediately replied that he had done so and confirmed the diagnosis by operation, he did not say laparotomy (*Brit. Med. Jour.*, Dec. 4th, 1886, p. 1094). Dr. Kelly also said the diagnosis rested upon the rapid enlargement of the sac, pain, expulsion of decidua, and *subsequent contraction of the sac.* The last sign seems a strange one in the ante-rupture period. Surely he referred to the contraction of the sac which occurs subsequently to fœtal death and absorption of the liquor amnii!

CONCLUSIONS AS TO THE DIAGNOSIS OF EXTRA-UTERINE PREGNANCY UP TO THE PERIOD OF PRIMARY RUPTURE (DURING FIRST THREE MONTHS).

To sum up the whole discussion, the symptoms are far from being definite and distinct. At the outset the sympathetic signs of pregnancy and the changes which occur in the uterus would tend to the idea of normal gestation. The woman may enjoy perfect health, or at least, better than she had in previous pregnancies. No reliance whatever can be placed on the cessation of the menses, as the discharge persists in about an equal number of cases, and in another class of cases irregular metrorrhagia is so common as to be actually looked on as of value in the diagnosis of extra-uterine gestation if associated with the sympathetic symptoms of early pregnancy. Very commonly abdominal pain is complained of from an early period. This may be intermitting but is more often constant and fixed in a certain part of the abdomen. But then it is often quite absent till rupture occurs. In many cases true decidua is discharged along with blood, and when present is of great diagnostic value. As the tumor grows, pressure symptoms may attract attention, as difficulty in defæcation and micturition, but these symptoms merely indicate pelvic trouble of some sort. Practically, the fact seems to be that in the vast majority of cases no opportunity for diagnosis is given. The women either have no symptoms, or their symptoms are not such as to lead them to seek advice. But certainly the diagnosis can be made and has been made.

In fact, in some few cases where the history does not, as it usually does, mislead, and where a number of symptoms are present, the diagnosis seems very easy. Still, in a far larger number it would be most difficult, whatever opportunities were given. Tait is, doubtless, too confident in his own unique experience, and too skeptical as to the assertions of others. We must remember that after all he has only seen seventy-nine cases (published up to date of his new book, December 1st, 1888) and this number is far too small to permit of any dogmatism as to what can and what cannot happen in other cases. There can be no doubt, however, that he is right as refers to the overwhelming majority of cases, and we may safely accept his last opinion on the matter, viz., "one might guess, but it is impossible to affirm; for one case we could diagnose, fifty would escape recognition." (*Brit. Med. Jour.*, March 10th, 1888, p. 536.)

Although I have a great respect for every opinion of Mr. Tait upon a subject he has made peculiarly his own, yet I do not slavishly follow whatever he may say unless it is well supported either by himself or others. But in his opinion on the practical impossibility of making an absolute diagnosis of extra-uterine pregnancy prior to rupture Mr. Tait does not stand alone. At the debate on the subject at the British Gynæcological Society, Feb. 8th, 1888 (*Brit. Med. Jour.*, March 10th, 1888, p. 536), Dr. Japp Sinclair said that in three cases with the details of which he was familiar, there *were absolutely no symptoms* before rupture; and in two of the cases even after rupture the symptoms were most misleading. Again, he had met with many cases which simulated tubal pregnancy. At the same place Dr. Rutherford said that before the third month they could not be sure they had to deal with a case of extra-uterine fœtation. It seems to me rather suspicious that those most famed for abdominal operations should, as a rule, express most doubt about the diagnosis of extra-uterine gestation prior to rupture, while those who go in for electricity, puncture, injections of morphine, etc., should profess to make the diagnosis offhand. Contrast this with the opinions as to diagnosis and treatment just after primary rupture. Here those who insist on abdominal section say the diagnosis is usually quite easy. Tait says we can make it seven times out of eight and guess at it the eighth time, while the stock argument of those who are against operation, the excuse under which they shelter themselves, is the impossi-

bility of anything like certainty in diagnosis. They feel if they admitted the diagnosis after rupture to be easy, they could then only plead want of skill in abdominal operations. Let us hear G. Ernest Herman, M.B., London, obstetric Physician to London Hospital, an obstetrician of skill and fame; a man who has done all abdominal operations with great success, yet a man who has not rendered himself prominent on either side of the question, who has neither denounced one particular mode of treatment nor unduly lauded another. In a very able lecture on the treatment of early extra-uterine gestation read before the Hunterian Society, London (*Lancet*, May 26th, 1888, p. 1021) he says: "There are a great number of published cases in which the diagnosis of extra-uterine gestation has been made, galvanism has been used, and the tumors have shrunk away. But the great mass of literature of this kind is far from convincing. The papers of Thomas (*Amer. Gyn. Trans.*, vol. vii, p. 219) and Garrigues (*ibid.*, p. 184) contain many cases in which the diagnosis rests upon such *slender grounds that it is impossible not to suspect error.* The strength of a chain is in its weakest link, and when one finds *seriously* adduced as *scientific evidence* cases of an *extremely doubtful* kind, without apparently a *sufficient sense of the possibility* of mistake, one is apt to judge of the strength of the whole chain of evidence by these *weak links.* There are cases, however, such as that of Dr. Petch (*Brit. Med. Jour.*, 1886, vol. ii, p. 1092), in which the fœtal heart was heard, and after electricity was used the cardiac sounds ceased. This case, so far as I know, *is unique.* But there are some others in which the diagnosis seems *highly probable*, and the tumors went away . . . . I think we want a larger number of cases diagnosed with much greater accuracy than the great bulk of those hitherto published, before it ought to be accepted as a fact that we can by a galvanic current kill an extra-uterine fœtus, and the evidence is not all on one side." He then cites the famous cases of Matthews Duncan and of Braxton Hicks where electricity failed. He then adds: "Now, in my judgment, a single well-observed case of this kind, in which the diagnosis and the effect, or rather non-effect, of the treatment were beyond all doubt, outweighs a great number in which the diagnosis was uncertain, and the possibility of natural cure by rupture or death of the fœtus overlooked." Remember, this is the judgment of a man who does not deny the possibility of the diagnosis. Indeed, in this very lecture

(*Lancet*, June 2d, 1888, p. 1069), he gives minute and accurate details of three cases at the London Hospital, where he opened the abdomen. Two of these cases were advanced far past the rupture period, and he honestly confesses he failed to diagnose either. The third was seen *prior to rupture*, and this he diagnosed correctly, as was proved by the ruptured tube and large hemorrhage, etc. To show his fairness, because he did not find the fœtus in this case which is not uncommon, he does not pretend to be sure of the nature of this case even yet (it was a two months' case), but remarks (p. 1070): "But if the diagnosis be assumed to be erroneous, the case is yet more interesting as one of unilateral dilatation of the Fallopian tube with *hæmatocele* and *subjective symptoms of pregnancy*. The whole tone of his lecture is that the diagnosis of extra-uterine pregnancy prior to rupture is difficult, and even impossible, from one circumstance or another, in the great majority of cases, yet easy enough in some. I believe the great stumbling-block in the way of a more frequent diagnosis is that we do not keep the possibility of the case being an extra-uterine gestation before our minds. Even famous gynæcologists and abdominal surgeons are too apt to have their minds fixed on ovarian cysts, the various forms of salpingitis, simple hæmatocele, and uterine myoma, with the result that when they turn out a fœtus it comes on them as a complete surprise. Some of them have confessed, indeed, that because there was no disturbance of menstruation, or because the patient was suckling, the idea of pregnancy never entered the patient's head, and consequently never entered theirs. Diagnosis by exclusion is the most certain and the most scientific plan known, but how shall we exclude a condition that we have never considered the possibility of? In diagnosis by exclusion we lay down a mental plan of all the diseases we consider the case before us may possibly be. It follows, that if we leave one out we leave ourselves open to an egregious blunder so far as that particular disease is concerned. That the difficulties are often very great, the most casual glance through recent medical literature proves. For instance, Wassilieff (*Centralbl. für Gynäkologie*, Dec. 26th, 1885) details the case of a woman, aged thirty, married for sixteen years, healthy, last menses on Sept. 25th. Two months later she came to the hospital suffering from great salivation, pain in abdomen, and anæmia. The case was diagnosed as parametritis and toxic gastro-enteritis (on account of the salivation). She died on Jan. 3d. On post-

mortem examination peritonitis of lower abdomen was found, and in Douglas' pouch a fœtus of four months. The umbilical cord was twenty centimètres long, placenta large. The fœtal cyst was situated in the left broad ligament in the angle between the tube and the womb: the latter was not enlarged. One would be inclined to say that here is a case where the possibility of extra-uterine pregnancy never flashed across the examiner's mind at all, or he might at least have got nearer to the true diagnosis. With pain in the abdomen, a cyst large enough to contain a four months' child, and its placenta in the pouch of Douglas, and salivation, one would think the attendant might have made a better guess than "parametritis," if the bare possibility of hæmatocele or extra-uterine gestation had ever passed before his mind. Again, in the Boston *Medical and Surgical Journal*, July 15th, 1886, we have a case by Dr. E. A. Kemp, where the patient, aged twenty-six, had been married three months; healthy and strong. On the evening of March 25th, she became faint and had to be helped to bed; complained of pain in the stomach with symptoms of indigestion. Next day patient still had the pain and was very pale. Had menstruated regularly, last time only over two days. Did not seem to rally, and Dr. Frost saw her in consultation; died at 2 A.M. next day. At the post-mortem examination the surface was unusually blanched, and the abdomen full of blood. Both iliac fossæ were full of clots, with a tubal pregnancy of the sixth week on one side. Dr. Kemp publishes the case on account of the absence of the signs of pregnancy and the perfect regularity of menstruation, which he considers remarkable. Had he been a student of Lawson Tait, he would not have been at all surprised at these matters. In fact, it is hard to say how he missed the diagnosis of at least "abdominal collapse," as Barnes calls it, and hemorrhage into the abdomen. I think Tait would have had little difficulty in complete diagnosis here, although he is so modest and distrustful of his powers previous to rupture. This brings me to remark that those who diagnose, or fancy they diagnose, early extra-uterine pregnancies depend on two things principally, viz., the presence of all the most important signs of ordinary pregnancy and the interruption of menstruation, if only for a period or two, of course with the presence of an extra-uterine tumor. Now, the ordinary signs of pregnancy and the menstrual suspensions may both be absent, in which case they are apt to fail

most disgracefully. So, it seems to be that those who know most of the natural history of the affection are most cautious and those who know least the most confident. I suppose this holds in all branches of knowledge. That difficult cases can be and are diagnosed is proved by the following case (*Centralblatt für Gynäkologie*, April 14th, 1888):—

On Friday, February 17th, Dr. Zucker, of Berlin, saw a woman, aged 32. As she did not speak German, no history could be obtained. She had borne children and missed at least one period. Abdominal pain set in severely on February 12th, but disappeared in two hours when purged. She went to work again, but pain again seized her on the 17th (morning). The uterus was found pushed to the *right*, and an obscurely circumscribed soft tumor could be detected to the *left* of the cervix. There was much tenderness. "Abdominal collapse" set in and she was sent into Dr. Veit's wards. At 2 P. M., on February 18th, he operated. Even under chloroform bi-manual examination failed to detect any distinct tumor. The patient was pulseless on operation, and when the abdomen was opened nearly four litres of fluid blood escaped. The *left* appendages were found quite healthy. The hemorrhage was at length found to proceed from a rent two inches long near the centre of the *right* tube. A minute ovum was discovered, which makes the nature of the case certain. The tube was ligatured on each side of the wound and the pedicle tied, but it was not said whether the appendages were removed. A litre of blood was left in the pelvis, Dr. Veit hoping it would be absorbed. A compress was applied to the abdomen and the patient rallied a little, but soon died. Here the diagnosis of abdominal collapse and hemorrhage was plain enough, and as the operator was well acquainted with the subject, and knew the most common cause of intra-peritoneal hemorrhage, he referred it to its proper cause, although the hæmatocele proved delusive as to the seat of the pregnancy. This case shows on what different principles diagnosis after rupture is made from that previous to rupture, and how much more certain it is. But I introduced this case for another purpose. Dr. Veit takes occasion to say in relating the above case that he has operated in ten cases of tubal pregnancy. Seven of these were operated on *previous to rupture;* all these recovered. The other three were operated on after rupture, and two of these died.

Now there is nothing surprising about the deaths, if the cases at all resembled the one he describes. If we wait till the patient is dying, the chances of recovery are small. But what I want to point out is that here we have seven cases diagnosed previous to rupture, and the correctness of the diagnosis proved by operation. Again, F. Westermark (*Nordiskt medicinskt Arkiv*, Band xix, No. 23) states that besides Veit's cases, Gottschalk has operated twice previous to rupture, both successfully. Westermark advocates laparotomy prior to rupture, but remarks that the chance is seldom afforded, for women do not apply for aid, as a rule, until serious symptoms arise, that is, when rupture has already taken place. These cases, however, prove that the diagnosis can often be made, provided the opportunity be given. Another case from modern literature, proving the possibility of correct diagnosis before rupture, and I have done. In the *Meditz. Obozr.*, Fasc. vii, 1885, p. 689, Dr. Maria O. Prujanskaia, of Moscow, gives the case of a woman, aged 29, who had been twice confined at period, last time six years since. Aborted in August, 1884, and again ceased to menstruate in November, when mammary symptoms, nausea and vomiting set in. At the beginning of December, when stooping to tie her boots, severe pain in the abdomen suddenly appeared; this was soon followed by giddiness, dyspnœa, fainting, nausea, and bloody discharge from vagina. These symptoms recurred at the middle of December and at the beginning and middle of January, 1885. The author was sent for at the beginning of the fourth attack; found the uterus slightly patulous, enlarged and softened. The fundus was a little pushed over to the right by a firm, elastic, indistinctly fluctuating tumor in the left side of the pelvis. The tumor could be felt through the roof of the vagina, left side. A number of the usual symptoms of pregnancy being present, a three months' extra-uterine gestation was diagnosed. The author proposed to kill the fœtus by injections of morphine or electricity. These being refused, the case went on until by the end of January fœtal movements were distinct. The further history does not concern us here; suffice it to say that finally the vaginal operation (elytrotomy) by Paquelin's thermo-cautery was done, with death the night after. Fœtus alive, 4½ months. Here, again, the diagnosis seems to have been made by the ordinary symptoms of pregnancy having occurred, the presence of a pelvic tumor and pain pointing

to its extra-uterine character. The principal advance in our knowledge of recent years in this department, viz, diagnosis of extra-uterine pregnancy before rupture, seems to be that we know now that there may be no symptoms whatever, no thought of pregnancy in the patient's mind, no arrest of menstruation, no pain, and if sympathetic symptoms of pregnancy are always present, they may be so slight as not to attract the patient's attention. Then there may be all kinds of menstrual disturbances—metrorrhagia, extrusion of decidua, and "false abortion." Probably a case presenting strong signs of pregnancy, with pain in the abdomen and an elastic, semi-fluctuating tumor at one side of the uterus, pushing it out of the middle line, with vaginal hemorrhage and passing of membrane, would have been diagnosed as an extra-uterine pregnancy many years ago. What we have to recognize and have always present to our minds is the possibility of extra-uterine gestation whenever we encounter a pelvic tumor in a woman who is within the fruitful age. If we keep the possibility before our minds, and remember that there may be no other symptom but tumor, we shall probably diagnose more cases in the future than has been the case in the past; at all events, our present knowledge should prevent the somewhat shameful missing of cases which ought to have been recognized, some of which I have instanced.

THE MISTAKE OF TAKING A NORMAL PREGNANCY FOR AN EXTRA-UTERINE GESTATION.

Extra-uterine pregnancy has on the other hand been diagnosed when the fœtus was intra-uterine. Playfair quotes a curious example from Joulin, in which Huguier and seven of the most skilled obstetricians in Paris agreed on the diagnosis of extra-uterine pregnancy, and recommended an operation, when the case terminated by abortion and proved to be perfectly normal. No explanation is given, but probably it was one of those rare and curious cases described by Tait ("Lectures on Ectopic Pregnancy," 1888, p. 62–3), who says that no other author except Parry has mentioned at all, viz., thinning of the uterine and abdominal walls, with the liquor amnii in abnormally small quantity, so that the fœtus is felt so very plainly that it is impossible to believe it is not free in the abdomen. As this curious state more directly concerns the diagnosis of extra-uterine pregnancy at the full time and afterward, I shall say no more about it here. Although I have

advocated, principally, a dependence on the results of bi-manual examination of the pelvis, still, I would point out every peri-uterine tumor, rounded or oval in outline and producing more or less displacement of the uterus forward and to one side (the opposite to the tumor), is not to be considered an extra-uterine pregnancy. There are various conditions which give rise to very similar swellings, such as small ovarian or fibroid growths and extra-peritoneal, or ordinary simple hæmatocele, pelvic abscess, etc. The differential diagnosis of these we shall consider separately. But that the danger of mistaking a normal pregnancy for an extra-uterine one is real may be proved by a case contributed by Dr. Blackwood to the *Philadelphia Medical Bulletin*, and quoted in *Brit. Med. Journ.*, 1886, vol. ii, p. 34.

The patient, the mother of two children, had menorrhagia in December, 1883, which was treated by faradism of the uterus. She menstruated normally early in January, 1884. Had connection on January 8th, only time for a month past. Intra-uterine treatment resumed on January 15th. Had severe abdominal pain in February. Fallopian tubes both discovered dilated; intra-uterine treatment then abandoned. Ten applications made to interior of uterus since menses, —uterus empty. Signs of pregnancy showed, but fœtus could not be found. By middle of March a round tumor the size of a walnut above right groin; mobile, not tender, and diagnosed to be a tubal pregnancy. Electricity employed up to 59 volts, to kill fœtus. In a few days the mass disappeared and patient became weak and prostrate. A fortnight later *tumor reappeared* but *more central in situation.* Sound passed again; uterus *seemed* empty and could be drawn away from tumor. By the end of April, the *fœtus which had been gradually coming nearer uterus entered it*, and internal os had closed. Normal delivery in the following December, six weeks beyond term—no complications. Dr. Blackwood thinks the electricity accounts for the six weeks the child was behind time, but does not attempt to explain the other curious phenomena. I think this case is an excellent example of a normal pregnancy mistaken for an extra-uterine one on account of some little swelling in the groin. To seriously bring the case forward as scientific evidence of *anything*, denotes a strong belief in the credulity of the profession. It teaches some lessons quite unintended by the author. It shows how hard it frequently is to produce abortion in a healthy woman, and it shows the amount of perfectly useless

and uncalled for local treatment women who can pay are subjected to in the present day. It also shows that electricity in the neighborhood of the uterus is not very dangerous to the life of the fœtus. In fact, the case reminds one of the reasons given by Dr. Steavenson for questioning the opinion that shocks from faradic currents would produce the death of the fœtus in early pregnancy. "The inhibitory apparatus of the heart was not developed in a fœtus three or four months old; and the *tenacity* of life *in a fœtus* was comparable to that which characterized the lower animals, such as tadpoles; and any current that was capable of destroying the life of the fœtus was much more likely to destroy the life of the mother" (*Brit. Med. Jour.*, December 4th, 1886, p. 1094). Even did we suspect some pelvic swelling to be an extra-uterine gestation sac, it hardly seems necessary to run the risk which Dr. Blackwood did by passing the sound periodically to see whether the uterus was empty. Rasch's method of detecting early uterine pregnancy by the detection of fluctuation as soon as a little liquor amnii is formed, which he says is available sometimes at the seventh week and always at the second month, ought to satisfy us on that point. (*Brit. Med. Jour.*, 1873, vol. ii, p. 261.)

IS THE DIAGNOSIS OF THE VARIETY OF EXTRA-UTERINE PREGNANCY POSSIBLE BEFORE RUPTURE?

I think all are agreed that it requires a careful post-mortem or ante-mortem section to determine what variety of extra-uterine gestation is present.

"We are not warranted, except in rare instances, in asserting that the ovum is developed in any particular portion of the genital canal, unless we have the opportunity of making a post-mortem examination."—J. S. Parry. Tait admits that the pavilion of the Fallopian tube might become the seat of a gestation; he accepts the tubo-ovarian as a possibility. Tubo-ovarian cysts are common enough, but Tait has never seen anything like a tubo-ovarian pregnancy. Did such occur the only point about it would be that it must of necessity always end by rupture into the peritoneal cavity, and that, probably, within the period which strictly limits the pure tubal variety, viz., thirteen or fourteen weeks. According to the new pathology the bursting of the tubo-ovarian variety would, almost without exception, involve death by sudden or slow hemorrhage and

shock, unless saved by abdominal section. Tait has seen twenty-six post-mortem examinations for ruptured extra-uterine gestations; has operated on forty-three, and has seen about ten such operations by other surgeons, making in all the unexampled experience of seventy-nine cases. In every one of these the seat of gestation was without doubt the Fallopian tube, and only one was in the part of the tube embraced by the uterine wall—the interstitial variety. The interstitial kind he argues must thus be very rare. All other kinds he has yet to see, except gestations (secondary) in the broad ligament as the result of rupture of the tube in the fourth of its circumference corresponding with the two folds of the broad ligament. Of course he has seen the ovum in the cavity of the peritoneum as the result of tubal rupture. The interstitial variety agrees with the tubo-ovarian in *always* bursting into the cavity of the peritoneum. Such cases have been stated to sometimes burst into the cavity of the uterus. But Tait has *never seen* a preparation of interstitial pregnancy which *could by any possibility* have been diagnosed from normal pregnancy before the period of rupture. He says it is much easier to believe that such statements are due to errors of diagnosis than that the tissue of the uterus is ruptured and that the pregnancy becomes intra-uterine. The period of rupture in the interstitial variety seems to vary from three to twenty weeks, a fact he derives from post-mortem record and museum specimens only. Tait believes that, as a rule, the interstitial form takes a longer time to rupture than that in the free part of the tube. The solitary case of interstitial pregnancy which Tait has seen was one on which he performed laparotomy for right-ruptured tubal pregnancy on the 11th of May, 1885 (*Brit. Med. Jour.*, Dec. 19th, 1885). She made an easy recovery. Eighteen months afterwards she had another child—nothing remarkable about the labor. About fifteen months after this confinement she again became pregnant—no unusual symptoms except slight pain in the abdomen, not enough to cause her to seek advice, even after her terrible experience. The only point she emphasized was that she felt the child more plainly than ever before at a similar period. On March 9th, 1887, while in the act of stooping, she was seized with acute pain and a feeling of faintness. Stimulants were given, etc., but the patient died 5½ hours after the seizure, of internal hemorrhage. The abdomen was found full of clots and fluid blood. The cavity in which the fœtus

was developed was separated from the true uterine cavity by a strong septum of uterine tissue. The true uterine cavity was lined with true decidua. The left tube communicated with the cavity of development in which the fœtus and placenta lay, and the rupture had taken place in the upper and back part of the left uterine cornu. Here we have the almost incredible instance of a woman having tubal pregnancy twice with a normal pregnancy between. Yet during neither of her abnormal pregnancies was there any symptom to cause her to consult a doctor until the collapse of rupture seized her. I conclude, then, that differential diagnosis of the varieties of extra-uterine pregnancy is totally impossible previous to rupture.

DIAGNOSIS OF EXTRA-UTERINE PREGNANCY AT THE TIME OF, AND SHORTLY AFTER RUPTURE.

The ovum, wherever it may develop, whether in the pavilion, the free part of the Fallopian tube, or in the interstitial part, as it grows distends the walls of the tube, forming a gestation sac. The chorion develops a placenta which penetrates the mucous membrane of the tube, encroaching on the hypertrophied muscular coat, so that where the placenta principally grows becomes the weakest part of the sac. Then, when the growth of the ovum exceeds the accommodating growth and stretching of the sac; when the stretching of the tubal sac reaches a certain point, spasm of the muscular wall is excited. Or, if spasm be present before, at length it detaches a part of the placenta, and bleeding occurs. Some of this blood usually escapes by the uterus, some is retained, increases distention, excites further spasm, and the end is rupture of the tube sac. Perhaps this process goes on many times before rupture takes place, which would account for the pain and hemorrhage which so often precede rupture. The ovum itself does not always burst when the tube does. As soon as rupture occurs fresh bleeding takes place from the torn tube wall—often it must be quite free. The blood already accumulated in the tube, along with the fresh flow, are now poured into the cavity of the peritoneum, in the vast majority of cases, causing shock and acute anæmia. When a Fallopian tube ruptures there are only two possible routes that the blood can take. If the tube give way between the folds of the broad ligament the blood will escape into the substance of the ligament, it will there be entirely outside the peritoneum, it

will, in fact, form an extra-peritoneal hæmatocele. In case of tubal pregnancy, it will form extra-peritoneal hæmatocele plus a ruptured or unruptured ovum. Now the amount of hemorrhage and the amount of shock in extra-peritoneal hæmatocele are moderate, as a rule, and the danger to life is not great. In the case of extra-peritoneal hæmatocele from tubal pregnancy, the ovum dies in a great many instances and the whole thing is gradually absorbed, no one, perhaps, suspecting what had given rise to the hæmatocele. However, if the tube bursts in any other part of its circumference except where the folds of peritoneum forming the broad ligament are, then the rupture must involve the peritoneal coat of the tube, and the blood is poured into the cavity of the peritoneum. This constitutes intra-peritoneal hæmatocele, and in tubal pregnancy, intra-peritoneal hæmatocele plus a living or dead ovum. Bernutz pointed out, in 1848, the vast difference between intra- and extra-peritoneal hæmatocele, but, even yet, most authors confuse the two. There is really all the difference in the world. If the blood is poured into the cavity of the peritoneum, it has free space there, the bleeding is encouraged because the blood meets with, and is diluted by, the lymph always present in the peritoneum, and easily caused to flow in abnormal quantity by any irritation. The result is that, except as the rarest exception, when a tubal pregnancy ruptures into the peritoneal cavity the *bleeding is fatal*, either immediately or in a short time. Tait says he has seen nearly eighty cases of intra-peritoneal hæmatocele, all resulting in death save those (with two exceptions) in which abdominal section was performed for the purpose of obviating death. ("Lectures on Ectopic Pregnancy," 1888, p. 31.)

It is easy to see how it is otherwise in extra-peritoneal hæmatocele. Here the blood is poured into the meshes of cellular tissue, an excellent medium for coagulation. Then there is only a limited space that blood can flow into. When the broad ligament becomes distended it actually makes pressure on the bleeding parts by resisting further distention. The only primary danger is that the broad ligament may give way and allow blood into the peritoneum, when the bleeding goes on and on till death happens. Tait has seen dozens of cases of broad ligament hæmatocele and has never met with a fatal one. (*Ibid.*, p. 31.)

A tubal pregnancy is bound to rupture; in the free part of the

tube it rarely delays beyond the twelfth week and may be as early as the fourth; in the interstitial part of the tube from the third to the twentieth week. This rupture takes two directions: into the peritoneum, which is the fatal form; and into the cavity of the broad ligament. The latter, or extra-peritoneal, alone gives all the cases which go on to the period of viability, all the lithopædia, all the suppurating cysts discharging into the bladder, rectum, vagina, and abdomen, and also all cases which by *secondary* rupture of the broad ligament into the peritoneal cavity are called "abdominal pregnancy." (Tait's "Lectures on Ectopic Pregnancy," 1888, p. 7.)

This little bit of pathology was absolutely necessary to prevent confusion and explanation at every turn. Let us now look at the symptoms and diagnosis of the intra-peritoneal bleeding, the fatal form. The injury sustained, as Barnes says, is compound. There is the traumatic violence attending the rent, and the sudden impression on the sympathetic centres producing shock, and the hemorrhage. The symptoms are also twofold: shock causes collapse, shown by loss of bodily heat, loss of all strength and energy, almost imperceptible pulse, intense paleness, vomiting, and often, in a very few hours, death. To this group of symptoms Barnes applies the term "abdominal collapse," and the name is a good one. These symptoms ensuing on rupture are so characteristic that violence and poisoning are the only things with which it would be easy to confound them. The whole thing usually happens so suddenly—some slight exertion, such as stooping at work, when a violent pain seizes the woman, she becomes cold, pulseless, collapsed, and is so often found dead or dying—that suspicions of violence are often aroused. Sometimes, especially if the unruptured ovum sticks in the rent in the tube, the hemorrhage may not prove immediately fatal. The woman recovers from the symptoms of shock, she may have no further attacks for a few days, when bleeding suddenly recurs with another attack of pain and fainting. This may be repeated several times before death occurs, but unless surgery steps in to her relief, death is all but certain. Some cases are so violent in the first onset that there is hardly time to do an operation, but, as a rule, death does not occur for several hours. The textbooks all describe recovery from the first shock as taking place sometimes. They then describe how the general signs of extreme anæmia show themselves; with the local symptoms of a feeling of an accumu-

lation of blood in the pelvis, retro-uterine hæmatocele, and how the patient may sink under the effects of continuous shock and hemorrhage. Also how, if she escape this danger, she has still another before her—peritonitis; how this may set in rapidly, with intense pain, swelling of the abdomen, pulse small and very rapid, temperature 100½° F. to 101½° F., and the drawn, anxious face met with in abdominal inflammations, and how she may recover even then. This is all true enough, but two things are confused. All these symptoms are rarely seen in *intra*-peritoneal hæmatocele; death rarely delays beyond a few days, and for the most part occurs inside twenty-four hours; but in broad ligament hemorrhage—*extra*-peritoneal hemorrhage, we may have all these symptoms—even a prolonged illness. The mistake arose through a false pathology. Where the fœtus lived after the rupture, about the third month, it was almost always found encysted. Collections of blood, whether from rupture of extra-uterine pregnancy or other cause, were found encysted also. So, as peritonitis was the only means by which a free fœtus or hæmatocele could become encysted, it was assumed that peritonitis must have been present. But even in extra-peritoneal pregnancy, although we may have inflammation and suppuration in the sac, still, even here, we rarely have peritonitis. The encystment is brought about by the fœtus, or the blood distending the broad ligament and converting it, at once, into a sac or cyst.

Parry had a glimpse of the truth, but the point escaped him. He says, quite correctly, "It has been stated, however, that peritonitis, by which means alone intra-peritoneal effusions can become encysted, rarely follows the rupture of an ectopic gestation." Again Parry says: "There are few things in regard to extra-uterine pregnancy which excite more surprise than the *rarity* with which *peritonitis is noted* upon examination after death from rupture of the fœtal cyst. The practical conclusions that may be drawn from a careful investigation of this subject are: That peritonitis is a rare sequel of rupture of the cyst, and even when pain, tenderness, and other symptoms of this affection supervene after the escape of the ovum, they do not necessarily indicate the existence of inflammation. *Peritonitis so rarely follows rupture* of an extra-uterine gravid cyst, that the *possibility* of its *occurrence need not be taken into consideration* in the decision of any questions relating either to prognosis or to treatment." Parry, like

all the older writers, argues against his own conclusions. He describes extra-peritoneal hæmatocele from rupture of a gestation cyst, and calls it *intra*-peritoneal, and then says it is so strange that peritonitis does not occur, although the hæmatocele gets a good cyst somehow. Here I should point out that I am not blindly following the opinions of Tait. Prof. Chas. A. L. Read, of Cincinnati, who himself agrees with Tait both as to pathology and treatment, points out that a great many authorities now agree with Tait as to the pathology of tubal rupture and the after effects. He says that in fact more have adopted the pathology than the practice, though if they accept the pathology he does not see how they can consistently refuse to follow the practice, which has astonished the world by its results. Schroeder ("Handbuch der Krankh. der weibl. Geschlechtsorgane," 7 Aufl. Leipsic, 1886), J. Veit (*Zeitschrift für Geburt. und Gynæk.*, 1884), and Kiwisch (quoted by Lusk: "Midwifery," p. 289) are among the leading Germans who agreed with Fritsch ("Dis. of Women," p. 289, New York, 1883), that "the most frequent source of the hemorrhage is the ruptured ovisacs of a tubal or other extra-uterine pregnancy." Among the gynæcologists, Tait, Imlach, Berry Hart, Thomas, and Emmet are among the most conspicuous who concur in the doctrine, while among the obstetricians, Lusk, Parvin, Barnes, Galabin, and Playfair may be mentioned.

The diagnosis of extra-uterine pregnancy at the period of rupture does not call for much further consideration. The case can nearly always be diagnosed as an abdominal hemorrhage, at least, and as the serious form, intra-peritoneal, is nearly always due to a ruptured gestation sac, the diagnosis is usually easy. Tait says he has never seen an intra-peritoneal hæmatocele that was not due to a ruptured tubal pregnancy. He believes the only other cause to be hemorrhage from some torn adhesions, or badly tied vessel after an abdominal section. Occasionally, but very rarely, other internal bleedings may eventuate in intra-peritoneal hæmatocele, such as ruptures of the liver or other abdominal organ, aneurism of some large abdominal artery, rupture of the ovary, etc. There is perhaps one point which ought to be noticed in the diagnosis, as it might prove useful in those cases where several attacks of hemorrhage occur before the case proves fatal. We must not expect to find a defined tumor caused by the blood. It is free and unlimited by any membrane. When contained in the rounded cavity of Douglas' pouch it bulges into the

vagina like a soft bag, but no defined tumor can be felt above the brim of the pelvis.

Should the rupture take place into the broad ligament, the case is not so plain. The symptoms may be very slight, or not noticed by the patient, as we find in many histories; or they may be nearly as severe as in the fatal intra-peritoneal bleeding. It is all a question of loss of blood. The symptoms will be those of ordinary (extra-peritoneal) hæmatocele. The essential diagnostic symptom between the two forms of bleeding is, that the blood is confined in the extra-peritoneal form, so that it forms a tumor which can be felt above the brim of the pelvis. Of course, the ordinary hæmatocele (extra-peritoneal) has many causes besides a ruptured tubal pregnancy: any sudden arrest of menstruation, or metrorrhagia, whether natural or the pseudo-menstruation so common after abdominal operations. The shape of the tumor is regulated by the broad ligament and pelvic fascia. Mostly the effusion is not great enough to be felt above the brim at first. When extensive the uterus is fixed and generally displaced forwards with a fluctuating swelling behind, or at one side. This swelling takes the shape of the distended broad ligament, concave beneath and rounded above the brim. It is not at all dangerous, even after abdominal operations, unless, indeed, the over-distended broad ligament gives way, and thus the blood enters the peritoneal cavity by a second rupture. For when the broad ligament ruptures into the peritoneum, just as in the case of the Fallopian tube, the bleeding is continuous or goes on at intervals, till death occurs, unless an operation puts an end to it. Of course, other dangers may arise in time from an extra-peritoneal hæmatocele; it may rupture into the peritoneum at any time; when due to extra-uterine pregnancy the ovum may develop and give rise to future dangers, and the cyst may suppurate, resulting in pelvic abscess. After extra-peritoneal hemorrhage has happened, as a rule, there is pain, faintness, a rise of pulse and of temperature. These symptoms gave rise to the older idea that when rupture of a tubal pregnancy did not kill by hemorrhage peritonitis occurred.

In the *New York Medical Record*, Feb. 25th, 1888, Drs. Taft and Gaillard Thomas report a case where apparently an extra-uterine pregnancy went on to full time without the tube rupturing. Dr. Taft has found records of eleven other cases where tubal pregnancy has

reached the eighth month without rupture. But with our present pathology such a thing is impossible. We must remember how difficult it is to determine the relations of an extra-uterine fœtal cyst at such a late stage of pregnancy. The probabilities are that in these cases the tubes had ruptured early, the ova passed into the broad ligament and the ruptured tubes continued to form the upper part of the cysts which were completed by the ligament. Another case reported by Dr. Chambers, of Sydney (*Brit. Med. Jour.*, November, 21st, 1885, p. 984), is more difficult to explain. This case was only at the thirteenth week when abdominal section was performed. The case is described as abdominal pregnancy; the cyst was found in Douglas' pouch, but what concerns us here is that the ovaries and *tubes* are distinctly described as *intact.* On June 10th she was seized with severe abdominal pain and extreme collapse. The operation was performed on July 18th. This looks very like the usual tubal rupture, but is it possible the tube could contract and heal in a little over a month so as to leave no mark? I think not. Either the tubes were not properly examined, or if they really were intact rupture must have occurred very early and contraction and healing had been very complete. In this way Tait explains a somewhat similar case reported by Maticki (*Monats. für Geburtshülfe*, Mai, 1860), where the uterus and tubes could be traced and where the attachment of the placenta had become almost wholly omental. Tait says he has seen all stages of such a process and so believes it possible. He says there would be nothing more remarkable about it than the well-established fact that by axial rotation an ovarian tumor may be twisted off its pedicle and grow entirely from the omentum, or, as he says he has seen, from the ascending colon. He says he has also seen this strange transplantation in all its phases and in all stages. He also instances the fact reported by Lecluyse (*Bulletin de l'Academie de Belgique*, 1869), that an intra-uterine gestation became abdominal by the ovum escaping through an aperture left in the uterus by the defective healing of the wound of a previous Cæsarean section. The placenta became attached chiefly to the small intestines, and the history gives no hint of a sudden rupture. (Tait's "Lectures on Ectopic Pregnancy," 1888, p. 15.)

I shall conclude this part of my subject by a good account of an undoubted case of extra-uterine hæmatocele due to rupture of a tubal pregnancy. It is not often we get such a clear account of such a case,

at such an early period, and verified by operation. Indeed, it is the fact that operations are now so frequent which is teaching a new pathology. The treatment is revealing the disease. Dr. O'Hara reports (*The Obstetric Gazette*, June, 1886), that on September 25th, 1885, he was called to see a woman who had been in good health until two hours previously, when she was seized with severe rectal tenesmus (Tait's symptom of hæmatocele in left broad ligament) and pain in pelvis extending down the right leg and arm. When he arrived he found the patient collapsed, almost pulseless, with shallow respiration and cold extremities. She was the mother of three healthy children, the youngest a year old. She had missed one menstrual period about a week before the accident, and considered herself pregnant. Hence the diagnosis was extremely easy. She was considered to have internal hemorrhage due to ruptured tubal pregnancy at the fifth week. Opiates and stimulants were given. Next day there were signs of reaction, and laparotomy, though discussed, was deferred. Five days later Dr. O'Hara found the abdomen greatly distended, but no tenderness on pressure. Resonance was general on percussion, except over the right flank. The vaginal walls were œdematous, and the anterior wall was thicker at the cervix and to the left. The cervix was *moderately soft* and *patulous*. The urine was almost black, and the temperature 99° F. Two days later marked jaundice appeared with occasional vomiting and purging of bile. A few days later a swelling was noticed on both sides and in front of the cervix, with a painless, sanguineous discharge, containing fragments of decidual membrane. Micturition was difficult and painful. Three weeks after the first attack a terrible flooding occurred, lasting for an hour, and followed by slight hemorrhage, rigors and pain. On the thirty-third day Dr. Parish performed laparotomy. The incision was made in the *median* line, and the peritoneal cavity was found *empty* and the peritoneum normal. The exploring finger showed that the mass was *external* to the peritoneum, and extended upwards from the left half of the pelvis to a level with the umbilicus. Another incision was then made along the line of original selection—*i.e.*, above the outer border of Poupart's ligament—by which means the mass was reached without wounding the peritoneum. The incision was large enough to admit two fingers. About a quart of coagula, fluid blood, and pus escaped. The cavity was washed out with antiseptics, and the wounds

closed, a drainage tube being inserted into the abscess cavity. The patient rallied well, and there was not much discharge. A bloody discharge from the uterus, however, set in. Two weeks after the operation the uterine discharge had ceased, but free bleeding from the wound occurred. The temperature rose to 103° F., but there was no pain on pressure. Twenty-four hours after the operation the patient was permitted to sit up, but fresh bleeding occurred with same symptoms as at first. As the hemorrhage recurred the cavity was opened up and cleaned out. An injection into the rectum escaped through the wound, but not *vice versâ*. Subsequently discharges of fæcal matter, and a small gall-stone escaped from the wound. By May 1st the patient was convalescent, a small sinus alone remaining. The communication with the bowel had almost entirely closed. Menstruation had recurred. The patient's ultimate recovery was complete. It seems, in this case, that both fœtus and placenta had either been absorbed or softened down into pus or débris indistinguishable from the other contents of the sac. The case is an excellent example of rupture of a tubal pregnancy into the left broad ligament. The ending is often far more favorable than this. Tait believes that both hæmatocele and ovum are absorbed in the majority of cases, so that there is nothing to distinguish such cases from ordinary every-day pelvic hæmatoceles.

DIAGNOSIS AFTER THE SHOCK OF THE PRIMARY RUPTURE IS OVER.

Should we not see the case until after recovery from the shock of rupture, should there be such shock, we have then, according to the new pathology, to deal with a hæmatocele in the broad ligament. In a great many cases the ovum dies, so that the case becomes a simple extra-peritoneal hæmatocele attended by no great danger and tending to get well. In very many cases both ovum and blood are absorbed in the course of time, so that recovery is complete. In fact, the only dangers under such circumstances are that the distended broad ligament may give way at any moment, when the blood must flow into the peritoneal cavity, and peritoneal hemorrhage, as a rule, goes on to a fatal issue if surgical interference do not prevent it. Or, the sac may inflame at any time, and even suppurate, so that the hæmatocele may result in abscess of the broad ligament, which is, of course, indistinguishable

from abscess arising from any other cause. In fact, most recent writers believe that extra-uterine gestations will account for most of the pelvic hæmatoceles we encounter. So that, I say, if the ovum be dead, there is absolutely no distinction or means of diagnosis between hæmatocele from rupture of a tubal pregnancy into the broad ligament and hæmatocele from other causes, as sudden arrest of menstruation. The history is all we have to go upon, and that, as we have seen, is far more likely to mislead than to guide. However, in a very few cases, the ovum is not killed at the time of the rupture, but goes on developing. Of course, it may die at any moment between this period and the full period of gestation. If, however, it lives, the diagnosis is just as it was before rupture; that is to say, we must depend on any signs of pregnancy present to distinguish the case from one of simple hæmatocele or other pelvic tumor. Then, if there are enough signs of pregnancy present to make the diagnosis of that condition *probable*, by a pelvic examination we can get evidence enough to make it *probable* also that the gestation is extra-uterine. For instance, we have the position of the uterus in relation to the tumor, in front of and to one side. Then the uterus is absolutely pushed out of the middle line of the pelvis; it is more or less fixed; *the cervix is always open;* and the cavity is empty, as may be ascertained by bi-manual examination and the fluctuation test. In justice, I am afraid that this is all we can say about the diagnosis of extra-uterine pregnancy previous to the period when the fœtal heart sounds become audible. But when the fœtal heart becomes audible, the aspect of the case changes immediately. If we can hear the heart, and find the cervix open and the uterus empty, *the case is clear.* There is no possible source of error if *we are sure* the *uterus is empty.* If we are sure the uterus is empty, a little pelvic search will usually discover the whereabouts of the fœtus.

DIAGNOSIS OF EXTRA-UTERINE PREGNANCY ABOUT THE VIABLE PERIOD.

As I have said, after the fœtal heart becomes audible, the case is clear; still many are the cases in which diagnosis of an extra-uterine pregnancy is made while the child is safe within the uterus all the time. So our principal duty here is to point out some of the dangers of a false diagnosis in the direction indicated. One of these dangers arises when fœtal outlines and movements can be felt more plainly

than is usual. This always does and always should suggest extra-uterine pregnancy, but the suggestion should not be allowed to go further or to become public property without sound evidence, which is only to be gained by rational examination of the pelvis. The false pathology of extra-uterine pregnancy which has existed so long, has so prejudiced the minds of medical men as to make them an easy prey to this mistake. The so-called "abdominal" pregnancy was supposed to be so common that whenever a doctor felt the fœtus more plainly than usual, he immediately thought it was loose among the intestines. The fact is now ascertained that real abdominal pregnancy is the rare effect of secondary rupture of the broad ligament (this is the evident explanation of Jessop's case, which is the only reliable one on record). The consequence of the prevailing pathology is that no author except Parry noticed this important source of error until Tait illumined this point, as he has done so many. Parry says: "I have met with an example of *thinning* of the abdominal walls, a few years since, which was exceedingly puzzling. I was asked by Dr. E. W. Watson to see a young woman, to decide the nature of an abdominal tumor, which was the size of a seven and a half or eight months' gravid uterus. Upon making pressure upon the enlarged abdomen, a fœtus was felt receding from beneath the fingers, against which it immediately rebounded. It was so superficial in its situation that it appeared impossible to believe that there was anything more than the skin of the abdominal wall interposed between the fingers and the child." Tait has seen eight cases where not only was there a "thinning of the abdominal walls," but an excessive thinness of the uterine walls also. Tait says he sometimes had the greatest difficulty in convincing the practitioners who called him in that the case was normal. He says that often the walls were no thicker than a single fold of towel. Sometimes the uterus was also displaced. In these cases one could forgive the medical men fancying the child was in the abdomen. Langley Browne, of West Brunswick, had a case where the thin uterus was extremely retroverted. In other cases there was retroflexion or latero-flexion. In such Tait confesses *patience* was necessary, to resolve doubts. He says, if with such a case any urgent symptoms existed, so that patience would be precluded, he would use the sound to see if the uterus were really empty. In all but one of Tait's cases there was a marked absence of liquor amnii. A curious point is that the cervix

uteri is usually thinned too, as much as the fundus, so that on examination the finger comes on the presenting part as if it were only covered by mucous membrane. And it is only by the greatest care that it can be ascertained that the os uteri is at the centre of the presenting part. Tait's eight cases were all under the seventh month except one. During the eighth and ninth months the walls of the uterus became thicker and the liquor amnii increased, and the cases became normal in every way, labor included. The exceptional case has not yet concluded, but is well advanced in the eighth month. Careful vaginal examination makes it clear the fœtus is intra-uterine, but Tait says that an abdominal examination alone would convince any one that the child was free in the abdomen. There are some other conditions from which extra-uterine gestation requires diagnosis during the life of the child. The chief are intra-uterine gestations, with displaced uterus, during the early months, combined with fibro-myoma or cystic disease of the uterus; and as a rare condition pregnancy in one horn of a bifid uterus. For the first case there are three safeguards, viz.: pelvic examination, common sense, and patience. Doubtful cases must often arise, and the man who would omit patience or time from his list of diagnostics, providing no urgent symptoms arose, would thereby prove his shallowness of intellect. With regard to pregnancy in one horn of a double uterus, just as in interstitial pregnancy, it must often be impossible for the most careful to diagnose it from normal pregnancy on the one hand, or from extra-uterine on the other; the latter remark does not apply to interstitial cases. If pregnancy take place in a very undeveloped horn, it is practically extra-uterine gestation, and will end in rupture just as tubal cases do, unless very early abortion terminate the case. If, on the other hand, the horn be well developed, the gestation may go on to term and end by natural labor. Should a double cervix exist, the case would be evident. Suppose the tumor was clearly one-sided, although evidently attached to the uterus, and that by bi-manual examination we detected a fundus uteri close to the tumor and towards the middle line of the body, we should suspect pregnancy in one horn of a bifid uterus, and, I think, we would be fully justified in using Hegar's or Tait's dilators and exploring the interior of the uterus with the finger. Of course, this would be at the risk of producing abortion, which would probably be the best thing that could occur, in the interests of the patient. But still we should be

careful and trust to time in the absence of any urgent symptoms, as probably more intra-uterine pregnancies are supposed to be extra-uterine than extra-uterine mistaken for normal. We should always bear in mind the words of T. Gaillard Thomas with regard to such cases when urgent symptoms suddenly arise; but there is now no occasion to let the woman die, thus making the event of such importance as to overshadow a medical man's reputation for life. Every country village and town cannot possibly have a leading surgeon within range, but for that very reason every country doctor ought to think the matter out and be prepared to open the abdomen on a moment's notice as an urgent, life-saving operation. The operation is not as dangerous as herniotomy, nor half so difficult, yet any surgeon will do herniotomy on emergency, whether he likes it or not, as a life-saving operation. But the surgeon must have planned it in his mind beforehand, reflected on how he would do it, and the difficulties and dangers he might meet with. If the mind be not familiarized with the idea of taking the responsibility of doing such operations beforehand, it will simply shirk the duty when suddenly presented, and the patient be deprived of—not the chance—but almost the certainty of recovery. In the 42 operations Tait has done for abdominal collapse and hemorrhage, which it is safe to say would *every one* have died with any other treatment, he has only had two deaths, making a mortality of 4.7 per cent. This is about the mortality of mild cases of measles, so that the most inexperienced, if a qualified medical man at all, ought to cheerfully face the operation *as a duty*. The fact is that it is the will that is wanting. Most ordinary medical men have tacitly agreed with themselves that they will not endanger their reputation by doing any risky operations, such as tracheotomy, for instance. Now, this is another quite easy operation, and any medical man worthy the name could do it right well if he only prepared his mind beforehand and resolved not to shirk it when it became a duty. T. G. Thomas says: "Very often we hear of physicians being blamed on account of diagnosis in those cases which die suddenly from rupture. Every medical man who countenances such a charge *demonstrates his want of experience*, or his want of professional loyalty, by so doing. *Very often there is nothing in these terrible cases to excite suspicion; very generally nothing to decide us* positively, *even when suspicion is excited.*" The late Prof. J. Thorburn, of Manchester, after an extensive experience of extra-

uterine gestation and of diagnosis in doubtful cases, sums up the whole question thus: "*Granted an absolutely certain pregnancy, with very strong suspicion* of its *being extra-uterine*, the *probabilities are nevertheless very greatly in favor of its turning out to be intra-uterine.*" If obstetricians would remember these words they would avoid making themselves ridiculous on many occasions.

Before the fœtal heart sounds become audible, and after fœtal death, extra-uterine pregnancy has to be diagnosed from quite a list of other conditions, but I prefer to consider them in detail when we come to diagnosis after fœtal death. I shall only allude here to the fact that extra-uterine pregnancy may cause retention of urine, because whenever a tumor gets behind the uterus from above it presses it downwards and *forwards* as well as to one side. When the cervix is pressed close to the pubes the uterus may press on the bladder and the cervix on the urethra, thus causing retention of urine. Now, this retention would probably direct our thoughts to retroverted gravid womb, in which retention is a prominent symptom, as it often is in small ovarian tumors and ordinary simple hæmatocele (retro-uterine hæmatocele). What I wish to remark here, then, is that retention of urine, which may occur in extra-uterine gestation, is far less common in that condition than in either retroversion of the gravid uterus, small ovarian tumors, or retro-uterine hæmatocele. Retroversion is very apt to be confounded with extra-uterine pregnancy, and here the ordinary signs of pregnancy are of no avail, as the retroversion contains a fœtus usually. Retroversion may be best distinguished by tracing the firm rounded body of the uterus by vaginal and rectal examination and finding it continuous with the tumor. Another good point is that the supposed tumor in retroversion is much lower, much nearer to the pelvic floor, than is at all usual in cases of extra-uterine gestation. As to small ovarian tumors they have probably given rise to more protracted illness; they do not interfere with menstruation; the uterus does not undergo evolution; the purple color of the vagina is wanting.

The case of gestation external to the uterus goes on with intermitting attacks of pain until we arrive about the full period, when we have some characteristic phenomena. Whether the fœtus dies at maturity or not—

Spurious labor is sure to come on. This consists in more or less

frequent and powerful uterine contractions, perhaps some vaginal hemorrhage with discharge of what decidua has hitherto escaped expulsion during the false abortions which may have occurred. Often the contractions of the abdominal muscles rupture the gestation sac and even the broad ligament (secondary rupture), so that blood and liquor amnii, if not the foetus, escape into the cavity of the peritoneum, which accident is, of course, usually rapidly fatal unless the surgeon steps in. Perhaps, more frequently, rupture does not occur, the labor pains continue at intervals until the child dies from effusion of blood into the substance of the placenta, causing asphyxia. In other cases again the child lives through, and after, this false labor. It has been known to be alive several months after full time; but, as a rule, it dies either before false labor commences or during its course. In any case, after from several hours to several days the labor pains gradually pass off and often *lactation* is established for a little. We shall now pass to a most interesting portion of our subject—

THE DIAGNOSIS OF EXTRA-UTERINE PREGNANCY AFTER FŒTAL DEATH—OR RATHER AFTER FULL PERIOD OF GESTATION AND FŒTAL DEATH.

When fœtal death occurs at maturity we, of course, lose the sound of the fœtal heart, and after a little, the uterine souffle also. There has been some discussion as to how long the uterine souffle continues after fœtal death. Some have maintained that it lasts as long as uterine enlargement does, but this seems to be a mistake, as uterine enlargement often lasts for months, until the placenta begins to be absorbed. That is to say, when the placenta is implanted partly on the back of the uterus, which is very often, the uterus remains enlarged until the placenta begins to disappear by absorption; but, as a rule, the uterine souffle disappears shortly after the fœtal heart sound. Observations have been made specially on this point by Dr. R. Petch, in his celebrated case of killing the fœtus in extra-uterine pregnancy by electro-puncture (*Brit. Med. Jour.*, Dec. 4th, 1886, p. 1092). In his case, though the fœtal heart sounds ceased on August 24th, the placental, or uterine souffle continued till September 7th, a period of fourteen days longer. That the bruit was not simply a sound conducted from the abdominal aorta, or other large pelvic vessel, he thinks is proved by the sound disappearing when it did; if it had been

simply conduction the sound would not have ceased so soon, as the tumor in that time had not shrunk sufficiently in size to remove the pressure necessary to produce conduction. This persistence of the utero-placental circulation is most interesting, both in diagnosis and treatment.

We may safely say that after the death of the child (should the case be first seen then) is the most difficult period of all in which to make a diagnosis. This may seem absurd, as we have a large, perhaps fully grown, child there to help us; but this child so alters when the liquor amnii begins to be absorbed that it can be taken for almost anything. First, then, we should, of course, pay great heed to the whole history, although here in particular it is usually most misleading, and we should never place the least reliance on the statements of patients any farther than they are backed up by physical signs in the pelvis or abdomen. The history as to absolute sterility, or sterility for a number of years, is of importance; then a sudden arrest of the menses, a gradual increase in size, false labor about the time of expected delivery, all have their value, and especially if there be *any show of blood during the false labor and a decrease of size afterwards.* These two latter points, if carefully inquired into, would usually keep us right, no matter how the remainder of the history might mislead. Campbell emphasizes these points while warning us not to put our trust in histories: "In many instances of the different varieties of misplaced gestation, the catamenia are suspended; frequently, however, they appear regularly in each of the early months; in some cases they flow at uncertain periods, and in other examples they are either profuse or limited in quantity. In many cases, at an uncertain period of gestation we have hemorrhage, uterine effusions, the extrusion of coagula, of bodies which resemble moles, or portions of the placenta. These appearances have frequently led to the belief that the patient has actually aborted, or that the ovum was originally not extra- but intra-uterine and had escaped through a rent in the uterus into the peritoneal cavity, the extruded body in either case being viewed as the placenta." If this be so, the history of menstruation, at all events, and this is usually made the chief point in the history, is not of much assistance.

The hemorrhage at false labor and the diminution in size afterwards are, however, *constant* (Tait). Then we have the condition of

the uterus, which is also *constant*, viz, the uterus is always intimately connected with the tumor, most often in front of it; it is always enlarged until the placenta is absorbed from the back of it, and the os and cervix are always open, more or less. If the case is seen shortly after fœtal death, the tumor will be soft, there will be more or less ballottement in it, and possibly some part of the child, a hand or a foot, may be recognized by rectum, vagina, or abdomen. Parry well says: "If the woman does not perish from rupture of the cyst during the first four or four and a half months of gestation, it is not likely that an opportunity will offer to inspect the body until at, or near, or even some time after, the close of pregnancy. . . . . Though the presence of an encysted fœtus is not incompatible with life, and even with comfort and usefulness, the woman who bears such a burden within her is in constant danger of the cyst taking on inflammatory action, which will greatly endanger and may even destroy her. . . . . If the patient is not seen until after the *death of the child*, the diagnosis of an extra-uterine pregnancy may be very difficult. Many years may have intervened before the woman comes under notice. Of course, if the cyst has opened into the bladder, bowels, or vagina, or a fistula has formed through the abdominal walls, there will be *little or no trouble* in arriving at a correct conclusion. Difficulty will arise only *when the cyst has not ruptured*, or, having opened into the bladder or into the rectum out of reach, it has not discharged any of its solid contents. Under these circumstances a correct conclusion can be reached by carefully sifting the clinical history. No point is too minute for examination. As a rule, it will be found that all such women have a firm conviction that they were pregnant when the abdominal tumor made its appearance. Though more than a score of years may have passed, they will not have abandoned the idea that they still carry a child somewhere in the abdominal cavity. Such women will nearly always give the history of *labor at or near term, attended* with uterine hemorrhage, and followed by the secretion of milk, after which they will assert that the abdomen diminished in size, and that this diminution steadily continued until the tumor reached the dimensions presented when the patient comes under observation. This association of phenomena is very characteristic, and when they are all present, erratic gestation should always be suspected. The *diminution* in the size of the *abdomen after labor* is a most *important symptom*." When the

liquor amnii is absorbed, the uterus becomes smaller and more mobile, as does the tumor itself. The hands and feet are often found above the brim of the pelvis and may often be recognized, but are closely imitated by little nut-like cysts of small ovarian tumors; or, pelvic examination, especially by the rectum, may make the nature of the case quite clear by the unmistakable recognition of some part of the fœtus. Dermoid cysts and cancer are the causes, however, of many mistakes. A pelvic cancer has often been quite confidently declared to be a fœtus. Indeed, if the cyst be compressed down into the pelvis, diagnosis, except by exploratory incision, may be simply impossible. Here I should warn the reader against the great danger of the aspirator, or Barnes' favorite puncture, as a means of diagnosis. Both these procedures are *more dangerous in abdominal or pelvic cases than abdominal section.* All modern literature proves this. Parry says that unless it has been decided to operate for removal of fœtus *immediately*, the trocar is utterly unjustifiable. "A few, but very few, women have long survived its use." Jonathan Hutchinson says: "The practice is in itself attended by great danger, nor shall I deal honestly with you or myself if I do not candidly admit that, with due care and patience, I do not think that paracentesis ought to be necessary in a case of fœtal tumor simulating ovarian dropsy." Mr. Hutchinson had a case of fatal peritonitis after puncture. Dr. Jordan punctured a woman who had no bad symptoms, but in two hours complete collapse came on. He says: "The doubts cast on my diagnosis, and the variety of opposing views in regard to the nature of the case, which unfortunately resulted in the use of the aspirator, were nearly the cause of the patient's death." I shall discuss the diagnosis from ovarian tumor and from pelvic cancer separately. When the liquor amnii becomes almost totally absorbed, the cyst and its contents become so jammed together and so intimately related to the contents of the pelvis that diagnosis often becomes an utter impossibility, or a mere guess if attempted. In such cases, fortunately, that modern diagnostic means, the exploratory incision, comes to the surgeon's aid, and as it is essential to any treatment worthy of the name its use is the more laudable.

DIAGNOSIS OF SUPPURATION IN AN EXTRA-UTERINE GESTATION SAC.

Ultimately, in the great majority of instances, the sac containing a dead ovum undergoes inflammation and suppuration, becomes con-

verted into an abscess and, like any other abscess, discharges its contents. This happens no matter at what period of gestation the fœtus may die; indeed, an immature fœtal sac usually suppurates sooner than where the gestation has gone on till term. After the death of the ovum the liquor amnii is absorbed, then the soft parts of the fœtus and the bones to a great extent follow, until only a much smaller tumor remains in the broad ligament, containing debris of fœtal tissues and bones. Sooner or later inflammation takes place and we have a pelvic abscess. Here, in order to in the least understand the various outlets this abscess finds for itself, we must discard the "encystment" theory of the older writers and accept of the broad ligament theory of Tait.

Dezeimeris was the first to discover that there could be an extra-peritoneal pregnancy. His facts were confirmed by others, but the rationale of the occurrence could not be got at till lately. Campbell tried to explain the facts of Dezeimeris away by the favorite "encystment" theory. He says: "It is difficult to comprehend how the ovum can insinuate itself under the peritoneum, which is reflected over the organs situated in the brim of the pelvis. Through time, certainly, the connections of the *original cyst* with the adjacent parts become so numerous that, *when superficially considered*, the ovum may *seem* to be enveloped by the layers of the broad ligament; but how it can pass under this appendage it is impossible to conceive." There is now, however, ample explanation of the how and the why, as well as plenty of proof of the correctness of the doctrine. In the *Memphis Medical Monthly*, March, 1888, Dr. R. B. Maury gives details of a post-mortem examination of a case which affords complete confirmation of the conclusions of Tait, Hart and Carter, as well as of Werth and Dezeimeris. "The pelvic organs were removed, and it was then seen, beyond any possibility of doubt, that the fœtal sac was entirely extra-peritoneal, that the gestation had originated in the right Fallopian tube, and had developed between the folds of the broad ligament, downward to the pelvic floor, laterally to the pelvic wall, and upward into the abdomen. The ovum, in its development, had lifted the peritoneum off the bladder and the anterior surface of the uterus, while the relations of the peritoneum to the posterior uterine wall and to Douglas' pouch were not altered. The sac extended quite to the pelvic and abdominal wall on the right side, but did not go beyond the left

cornu of the uterus on the left. At the time of the operation it was observed to be covered by peritoneum, and this was clearly shown after death. The gestation was therefore entirely extra-peritoneal, and belonged to the variety intra-ligamentous of Werth, or sub-peritoneo-pelvic of Dezeimeris. This autopsy corroborates the view taught by Mr. Lawson Tait, that in extra-uterine pregnancy, no matter where the fœtus may be found, its development begins in the Fallopian tube, and that it may become intra-peritoneal or extra-peritoneal, just as the tube happens to burst." If this description had been invented by Mr. Tait to bolster up his views, it could not have been better fitted for the purpose. But in order to really understand the pelvic abscess resulting from suppuration in an extra-uterine fœtal cyst, we must also remember that according to the same view all the ova that survive rupture are certain to be situated in the broad ligament.

When a cyst containing fœtal débris suppurates, the matter *always* takes one of four directions: through the rectum, which is much more common than any of the others; through the posterior vaginal cul-de-sac, which is next in point of frequency; occasionally through the bladder; and most rarely of all through the abdominal wall near the umbilicus. Fœtal remains have also been known to pass into the colon or intestine, and through the uterus. Such cases are due to implantation of the placenta on those structures, with its subsequent partial necrosis and partial absorption. Tait says he himself did not understand how fœtal remains passed by the umbilicus, but the researches of Berry Hart give the reason as plainly as daylight. In such cases the peritoneum is lifted off the anterior abdominal wall as high as the umbilicus, and the cyst is in close relation with the anterior abdominal wall. Those who advocate a masterly policy of inactivity in regard to treatment represent this elimination of the fœtus as a very safe mode of getting rid of it. All we have got to do is to assist at the delivery of each bone, enlarge openings where necessary, etc.; but besides the years of misery, uselessness and suffering which women have to undergo during the elimination of the dead fœtus, at least a third of them die during the process, from putrid absorption. Parry has collected 330 cases; of these 105 died, which is probably under the truth rather than over it. The direction which the abscess takes depends on the seat of the original hæmatocele and ovum; this is a further proof of the truth of the theory. Effusion into the left

broad ligament dissects the peritoneum off the rectum, causing annular stricture and sometimes complete obstruction of the rectum; so we would naturally expect the fœtal débris would take the same route. Tait says he has proved the seat of the sac to be in the broad ligament by his finger in the rectum and a sound in the bladder, in many such cases ("Lectures on Ectopic Pregnancy," p. 57). We would expect the posterior arch of the vagina and the bladder to be the next easiest outlet, and in cases with a vaginal opening Tait has again proved the fœtal cyst to be in the ligament. He also relates the case of a lady who had been passing phosphatic calculi for years, the nuclei being the bodies of fœtal vertebræ. He opened the abscess from above the pelvic brim without opening the peritoneum, removed fœtal bones, hair, pus, and phosphatic débris, which resulted in immediate cure. He could put his finger into the bladder by an opening on its right side; and as the womb was quite fixed, he could not doubt that the abscess was the consequence of a dead ovum in the right ligament.

The diagnosis of these cases often cannot be made at all till a fœtal bone passes or gets arrested in the opening. The history is not to be depended upon unless the pregnancy has gone on till near term. If the fœtus die early, there is only a history such as we might have in any pelvic abscess. These cases cause tedious, painful and dangerous illnesses, and render the woman a useless invalid often for years. The vaginal form suffers less than the others, and the elimination is sooner over. Discharge by the bladder is far more dangerous than any other form, because here we have the danger of cystitis leading to surgical kidney, while the phosphatic encrustations of the fœtal remains greatly increase the pain.

This abscess in the broad ligament may follow any extra-uterine gestation which is deposited in the broad ligament as the result of primary rupture, which practically means rupture of the Fallopian tube; but the earlier death occurs in the fœtus, the smaller will be the abscess and the less the trouble, danger and pain in eliminating the contents. The older the fœtus previous to its death, the larger the abscess and the more trouble the bones have in finding their way to the surface.

Broad ligament hæmatoceles may suppurate, whether dependent on extra-uterine pregnancy or not, and in the early stage, before any bones are formed which are permanent enough to give trouble; of course, any differential diagnosis is impossible. Such cases pass as ordinary cases

of pelvic abscess. Tait thinks suppuration in hæmatoceles, taken generally, is not very common, yet many cases are encountered. The history might obscurely point to the origin of the hæmatocele, but, by the time the hæmatocele was merged in the pelvic abscess, probably the whole history would be merged in an obscure account of pelvic pain and trouble. About thirty pelvic abscesses have been operated upon by Tait without a death, of which he believed the origin to be hæmatocele, as he found considerable quantities of laminated, broken-down blood-clot. All these were extra-peritoneal, and he suspects about half of them originated in extra-uterine gestations. He gives the history of one case which is typical ("Lectures on Ectopic Pregnancy," p. 36). She was 45 years of age; had never been pregnant, except one doubtful miscarriage nineteen years before; symptoms like those of hæmatocele had occurred eight months before, after three months' suspension of the menses; she had been getting thin; had lost her appetite; had constant thirst and night-sweats, and abnormally high temperature at night. Uterus was fixed in a mass of effusion involving left broad ligament and part of the right. The mass on the left surrounded the rectum, forming annular stricture, as Tait has observed hæmatoceles on left side often do. *No other writer has noticed this sign of left hæmatocele.* No fluctuation could be felt in pelvis, but the symptoms pointed clearly to presence of pus in pelvis. The pelvis was opened from above; a large abscess found just behind base of bladder, between which and uterus it lay, but extended round behind rectum. The floor and posterior wall of abscess consisted of old laminated blood-clot; so its origin was a hæmatocele, extra-peritoneal. She went home cured on the thirtieth day.

As to the diagnosis of extra-uterine pregnancy at periods more remote, it must be conducted on general principles. Where the pregnancy has advanced to term, the history of pregnancy is usually clear enough. In such a case the history is quite different to that in cases at the period of primary rupture, or of hæmatocele, or pelvic abscess following primary rupture, where the signs of pregnancy had never been properly developed. Without the history of pregnancy it would be impossible to say what the nature of such a pelvic tumor might be. But as the retention of a fœtus or of fœtal remains is, and necessarily must be, a source of danger, to the dying day of the longest life, the exploratory incision will be justifiable, and will provide both the

means of diagnosis and of cure at the same time. The most recent case of lithopædion with which I am acquainted was related by Dr. A. Martin, to the Obstetrical Society of Berlin, November, 1885. A woman, 52 years of age, multipara, noticed signs of pregnancy in August, 1872. In 1873 she appeared as an out-patient, and a diagnosis was made of extra-uterine pregnancy with a living child. The patient declined to submit to any operation, and the child ultimately died. In November, 1885, twelve years after, she returned, and was willing to submit to any operation to be relieved of very distressing abdominal symptoms, which were rapidly becoming worse. The extraction of the lithopædion, which was lying on the right kidney, was easy enough; but great difficulties were experienced in extirpating a tumor which was adherent on every side, and filled the right half of the pelvis. The Fallopian tube was found distended with pus, and situated in a funnel-shaped cavity, which probably corresponded with the original site of the placenta and ovum. No trace of placenta could be discovered. Recovery took place without a bad symptom.

In the *Arch. de Tocologie*, April 10th, 1887, we have a most interesting and instructive account of the sequel of an extra-uterine pregnancy, which illustrates the dangers of retained fœtal structures very well. A woman, aged fifty, who had never been pregnant, suffered twelve years before from violent and unexplained crises of abdominal pain, which obliged her to keep her bed. Her doctor attributed these attacks to disease of the ovaries. The patient, however, firmly believed she was pregnant, and at about the full time, as she thought, the pain came on with unprecedented vigor. The doctor who was sent for waited for labor to take place for three days, and then, *without apparently a suspicion of the condition of affairs* (no nice diagnosis there) left, with a diagnosis of "dropsy of the ovaries." As subsequent events showed it was a case of extra-uterine pregnancy, but at the time this was not diagnosed; and three years later the patient had a huge abscess, from which were extracted fragments of fœtal bones. After the lapse of a considerable period of time the abscess healed and a second formed on a buttock, a third in the front of the hypogastrium, and a fourth in the perinæum. They all ultimately healed, but not until anchylosis of the hip joints had taken place. The last fragment of fœtus was a black, porous and friable scapula.

This took five months to effect its exit, and left a wound which took two years to close.

Dr. Notta relates another warning case in the *Progrès Méd.*, May 10th, 1884. Patient aged forty. Eight years ago a tumor began to grow; grew for a year, and then remained stationary for seven years. On admission she had all the symptoms of intestinal obstruction, and an abdominal tumor. Laparotomy was at once performed. The anterior abdominal wall was closely adherent to the sac, which contained a yellowish, oily liquid, and had no pedicle. Its walls were fibrous and 1½ cm. thick. The intestines closely adhered to the sac, and a strangulation was discovered. Death on the third day.

At the autopsy no peritonitis was found, but a second strangulation was discovered. The pregnancy was "abdominal," the fœtus had undergone fatty degeneration, the skeleton was unchanged, the membranes were partly calcareous. The placenta was not identified. The degree of development and position of the fœtus are not mentioned.

A case by Dr. Angus Macdonald (*Edin. Med. Jour.*, February, 1884, p. 697), should prevent us despairing of any case. As it is so instructive in every way, I give a sketch of it. Aged twenty-eight; one child six years ago; *no history of pain till sixth month,* when she had deep pain in the lower abdomen, with great difficulty and pain in passing water. Three weeks later a sanious vaginal discharge, and œdema of left leg beginning at ankle. In the seventh month hectic fever; uterus five inches long, depressed; fullness in Douglas' pouch and on both sides of pelvis, bulging into rectum, from which blood and pus flowed. An abdominal mass rose nearly to the umbilicus; breath sweet. The symptoms of septicæmia subsided, but much pain was felt, and threatening symptoms appeared on sitting up. At about the eleventh month operation was decided on. The macerated fœtal remains were found attached to the right and posterior abdominal parietes. *The cyst was formed by six inches of almost gangrenous intestine, and contained fœces.* A fæcal fistula was also found. Resection of this part of the intestine was performed, and drainage ended the operation, which was performed with antiseptic precautions *without the spray.* Fæces flowed through the wound for a few days only. Three months after the operation the patient was quite well, and no trace of deposit could be felt. *Diagnosis:* abdominal pregnancy, death of fœtus at end of sixth month, inflammation of sac,

peritonitis, thrombosis of left iliac vein, absorption of putrid contents of sac, and septicæmia, bursting of sac into bowel. Surely after the success of an operation even dealing with a sac containing fæces and complicated by resection of half a foot of gangrenous gut, we might enter on the simple section which is alone necessary to remove the ruptured tube at the end of the third month, without much nervousness as to the result.

At the Royal Medical and Chirurgical Society, London, on October 26th, 1886, Dr. Henry Gervis reported a most interesting case, the only one on record in which the fœtal sac communicated with both small intestine and uterus. I shall give a sketch of the case, and of the remarks by the members on it. (*Brit. Med. Jour.*, Oct. 30th, 1886, p. 818). A woman, aged thirty-five, was admitted to St. Thomas' Hospital on Nov. 19th, 1885. She had ceased to menstruate on May 9th, and her pregnancy proceeded without incident until July, when she experienced severe pains in the abdomen, which continued until admission. In September she passed by vagina a pint of thick, greenish fluid; similar discharges continued at intervals until she came under observation. There had been no uterine hemorrhage, but a piece of skin was said to have come away with the discharge on one occasion. On admission, an ill-defined swelling was noticed in the hypogastric region, extending toward the right of the middle line; the lower segment of the uterus appeared expanded and fixed. A large quantity of offensive discharge, yellow in color, was issuing from the vagina through the os uteri; this discharge was found to consist of broken-down muscular fibre and vegetable tissue. On Dec. 14th she passed, by the vagina, the shaft of a fœtal tibia. On exploration, the decomposing body of a small fœtus was found lying, as it were, transversely in the uterine cavity. It was tightly fixed in its position, and all attempts to extract it were in vain; slight hemorrhage occurred during the operation, and the patient died on the following day. On post-mortem examination, the great omentum and the intestines were adherent to the brim of the pelvis, and, under this mass the remains of a partially disintegrated fœtus were found, in the posterior half of the pelvis. Almost the whole of the posterior wall of the uterus had sloughed away. Dr. Gervis pointed out that he was unable to find any similar case of extra-uterine gestation occupying Douglas' pouch, communicating both with the uterine cavity and the

small intestine. Dr. C. H. Routh suggested that, as a rule, the only conclusion which could be arrived at when there was evidence of a *growing tumor* not in the uterus, with signs of pregnancy, was that there must be extra-uterine gestation. He had found it an invariable rule in extra-uterine pregnancy that the *catamenia ceased at once, but came on again in about three months;* and that pieces of decidua then came away. In this case, such a piece of decidua was probably passed. He thought that if a sponge or sea-tangle tent had been used, followed by a Barnes' bag, it would have been possible to reach the cavity of the uterus and remove the fœtus. Dr. Gervis, in reply, said the tumor was not a growing one when it came under observation. The first diagnosis was extra-uterine gestation, but this was rendered doubtful by the passage of portions of the fœtus from the uterus by the vagina. The interest of the case was the unique direction taken by the fœtus (through the uterus).

No doubt the fact of portions of the fœtus passing by the vagina from the uterus would shake the opinion of the most confident. The case illustrates the value of Tait's rule to trust no history, or statement, or theory any further than you can get physical evidence in the pelvis to back it up; and even if the case was supposed to be intra-uterine, finally, the treatment seems weak and vacillating. The indication was to remove the fœtus wherever it may have developed and in whatever way might prove necessary; but it was left sticking within reach and after efforts had been made to dislodge it. One fancies the treatment would have been much more decided and firm in other hands and, perhaps, with a different result.

DIFFERENTIAL DIAGNOSIS OF EXTRA-UTERINE PREGNANCY.

Probably the following list as given by Prof. Thorburn contains all the conditions that have been and are liable to be mistaken for extra-uterine gestation: 1. Intra-uterine pregnancy; 2. Normal pregnancy with retroversion or retroflexion; 3. Pregnancy in one cornu of a bifid uterus; 4. Ovarian tumor; 5. Cyst of broad ligaments; 6. Fallopian distention; 7. Uterine tumors; 8. Pelvic hæmatocele (simple); 9. Pelvic inflammatory exudations or abscesses (simple); 10. Cancer of pelvis or peritoneum.

1. *Intra-uterine Pregnancy.*—During the first two or three months it is very rare that anything unnatural is suspected. It is extremely

seldom that the medical man has any opportunity to attempt the diagnosis. As a rule, the opportunities for diagnosis commence after primary rupture in the very cases in which this does not carry the patient off, or lead to an operation which removes the pregnancy. I have already discussed the diagnosis from intra-uterine pregnancy so fully that much here is not required. After three or four months the occurrence of occasional metrorrhagia, or the return of natural menstruation with an increasing tumor, or frequent or constant pain, may lead to an examination. The practitioner should insist on an examination on hearing of any such symptoms. Then a good knowledge of the signs of pregnancy, a knowledge of what can be ascertained by bi-manual examination of the pelvis, and great care in forming his opinions, are the guides to success and the lights to warn from disaster. He should not build too much on any one symptom; he should not lay too much emphasis on the lateral situation of the swelling, especially if to the right, unless there are corroborative *physical* signs. In the advanced stage (seven or eight months) abnormal thinness of the uterus and abdominal walls is a great source of confusion, but then there is in almost all such cases an utter absence of any corroborating evidence, and bi-manual examination, as usual, is our only guide and safety.

2. *Retroversion or Retroflexion of the Gravid Uterus.*—Retroflexion or retroversion of a gravid uterus has a definite history not so liable to mislead as the history of extra-uterine gestation. There has probably been previous displacement of the uterus which the patient may be aware of, or if retroversion occur during pregnancy the history is definite and clear. There is a sudden onset of symptoms of pelvic pressure, obstructed defæcation, urination, pain in the pelvis, and *no vaginal hemorrhage.* If we only employ the old vaginal examination, we might easily be misled, but an intelligent man, who makes a careful bi-manual examination, can scarcely be deceived. The point is to establish the continuity of the uterine cervix with the mass behind, which always lies lower, nearer the pelvic floor than is the case with an extra-uterine tumor. Then the results of treatment complete the differentiation. It may be added that though difficulty of urination may occur in extra-uterine gestation it is never so prominent as in retroversion of a gravid uterus.

3. *Pregnancy in one Cornu of a Bifid Uterus.*—I have already

touched on this subject and have only to repeat that unless the horn be well developed this is practically an extra-uterine gestation, only more dangerous, as whereas a tubal pregnancy has a chance of rupturing into the folds of the broad ligament where it is quite extra-peritoneal and comparatively without danger, the gestation from a ruptured horn, unless removed through the uterus by very early abortion, is sure, on rupture, to be extruded into the cavity of the peritoneum, which practically means abdominal section or death. Unless we can detect a double cervix or os uteri, bi-manual examination is the only guide, and it must be confessed that such cases are most difficult to diagnose. Moreover, as in tubal pregnancy, there is usually no opportunity for examination till rupture occurs without the least warning. Fortunately, abdominal hemorrhage is easy enough to diagnose and not really difficult to treat. The only definite hint that can be given to guide the bi-manual examination is that a decidedly one-sided tumor closely attached to the uterus with a fundus at its side ought to make us very suspicious; and if bi-manual examination of the uterus and search for fluctuation in it give no signs of an ovum there we should insist on dilating the uterus and exploring its interior with the finger. This treatment would probably cause the gestation to abort, the only hope of avoiding rupture and abdominal section.

4. *Ovarian Tumors.*—This disease is very liable to be mistaken for extra-uterine gestation. The mistakes would be frequent were extra-uterine gestations going beyond primary rupture frequent. The history of ovarian tumor is more definite than the history of extra-uterine pregnancy. But it must be confessed that in many cases time and repeated bi-manual pelvic examinations are the only means of distinction. Some cases of extra-uterine pregnancy could be diagnosed from ovarian tumor at once and with ease; in other cases it would be totally impossible, at one visit, to the most skillful.

If we happened to have most of the symptoms of even early pregnancy present and a vascular, *growing*, semi-fluctuating tumor at the back or side of an enlarged uterus, with suspension of the menses for a time or two, and then irregular hemorrhages with severe crampy pain, the diagnosis would be evident to the simplest. In fact, as soon as symptoms of life, of real pregnancy somewhere, arose, the difficulty would be over to any intelligent man. But either before definite signs of life in the tumor, or after these have disappeared, diagnosis is

practically impossible in many cases without resort to a means which ought to be the more used, as it is also the only treatment worth anything, as a rule, that is abdominal section. This has now been proved to be less dangerous than puncture by aspirator or trocar, and with the progress of another twenty years is bound to be used by everybody. No doubt the difficulty between extra-uterine pregnancy or its results and ovarian disease is very rare; this is fortunate for the reputation of medical men. However, rare things seem to be always turning up to the annoyance of practitioners. I think the principal reason of the confusion and helplessness is that we allow the possibility of extra-uterine gestation to slip out of our mind when we are examining a pelvis to ascertain the nature of some tumor, hence the difficulty. The question does not arise in the mind thus—is this an ovarian tumor or a dead ovum? But is this tumor a cyst of the ovary, broad ligament, sub-peritoneal uterine tumor, cancer, etc.? Consequently it is impossible to diagnose what has never even received consideration. If we admitted the possibility of the presence of a dead ovum any evidence in that direction would be duly weighed and might at least prevent us making a false diagnosis by preventing us from making a positive one at all. Then on section for *diagnosis* and treatment we should not be thunderstruck by turning out a fœtus of which we had never thought. I am convinced that we all, consciously or unconsciously, diagnose in all cases by exclusion. We may exclude quite a number of things by half a second's reflection, but still this must be done if we wish to avoid blunders. I believe the difference between the man who makes many blunders and the man who makes few (who does not make some?) is that the latter takes a more extensive mental survey of the *possibilities* of the case: the former merely reviews a few of the more common, and thus judges on imperfect evidence. Defective diagnosis, especially when positive, usually arises from error in the management of the understanding more than from any other cause. We instinctively recognize this at the moment the blunder is exposed, hence the feeling of shame. If we had judged fairly on all the evidence obtainable why any shame? Hence, I repeat, bi-manual examination and reflection are the only beacons against error. Prof. Thorburn ("A Practical Treatise on Diseases of Women, London, 1885, p. 477) tells of an operation at a case of this kind by Dr. Atlee, in 1867, Sir Spencer Wells, Dr. Nicholay-

son, of Christiania, Drs. Burpee and Drysdale, of Philadelphia, and Dr. Atlee's elder brother, were present, and acknowledged that they had never been called on to diagnose a case of the kind.

5 *and* 6. *Cysts of the Broad Ligament, and Distention of the Fallopian Tube.*—The point here, as in the last case, is to give the possibility of early extra-uterine pregnancy or of a dead ovum a chance of being considered in our minds. We may say that where the symptoms of pregnancy, early or advanced, are present error would be almost impossible if any care were taken. But just as in the case of ovarian tumors, many cases might arise in which patience or abdominal section, according to the urgency of the symptoms, would be the only possible means of diagnosis. In pyo-salpinx there would be a history of pain with fever, etc., quite different from anything we could have in an early tubal pregnancy. Except affections of the tubes the other possible pelvic cysts agree pretty closely in their characters with ovarian cysts. The great point is not to forget the possibility of an extra-uterine ovum. These pelvic cysts are not, as a rule, urgent in their symptoms, and if we have the least doubt as to the possible presence of any form of pregnancy that doubt itself would save us from blunder, as we would wait until time would tell. In case of a dead fœtus, past term, we could hardly be said to blunder if we had carefully considered the possibility and then made an incision to see what it was. These cysts may, of course, be combined with extra- or intra-uterine pregnancy and with ovarian disease in the most puzzling and confusing manner. It would take a special treatise to point out and consider the possible combinations. But still a careful bi-manual examination of the pelvis and some sense will guide the most inexperienced past blunders almost as successfully as the greatest experience, unless combined with the greatest care and caution. When in doubt wait, at least till the possibility of *normal* pregnancy can be eliminated, then, if possible, have the tumor removed, except it be evidently an extensive cancer.

7. *Uterine Tumors—especially Sub-peritoneal.*—These are not very liable to be mistaken for extra-uterine pregnancy except the latter is suspected to be in the early stage. Consequently it is the small tumors which are most liable to be suspected. But these are always solid and have none of the semi-fluctuating feel of an ovum in the Fallopian tube. Then, they do not increase at the rapid rate an ovum does. The history usually has a longer duration than would be com-

patible with extra-uterine pregnancy, and is marked usually by metrorrhagia for a length of time. In fact, it is hard to understand how with bi-manual examination and a little common sense and prudence, a mistake could be made between the early stages of extra-uterine gestation and a small fibroid of the uterus. Sometimes, indeed, the metrorrhagia of extra-uterine fœtation previous to rupture might suggest myoma, but the myoma would be an interstitial one or a polypoid, so that a tumor at one side and behind the uterus, of a painful, fluctuating nature, could hardly be twisted into a myoma causing the metrorrhagia.

But it is a very different story in the advanced stage, or rather when extra-uterine gestation is over and the remains of the fœtus are consolidated into a mass close to the uterus, when all liquor amnii has been absorbed and nothing characteristic of a fœtus remains. This constitutes one of the most difficult cases for diagnosis which could be imagined, and a diagnosis of fibro-cystic tumor of the uterus is very generally made, often as a mere cloak for conscious ignorance. Those who, on paper, can diagnose anything should listen to the words of Parry on this subject: "Although from the careful perusal of numerous histories of cases of this nature some degree of facility of distinguishing their presence may be acquired after a certain period of their duration, and of deciding even, in *occasional* instances, on the particular variety of such pregnancies, yet assuredly every practitioner who *has attentively studied the subject* must admit the distinction to be a task of no ordinary difficulty." In fact, often there only remains the exploratory incision as a means of diagnosis.

8. *Pelvic Hæmatocele.*—In the vast majority of cases it must be impossible to distinguish between a hæmatocele arising from other causes and one the result of the rupture of an extra-uterine gestation. As the accident always takes place before the fourteenth week, the ovum, or fœtus, if the ovum be also burst, is too small to be felt among the clots in the pelvis. Fortunately, the differential diagnosis between simple hæmatocele and that from ruptured tube is not of the least consequence to treatment, or in any other respect, except perhaps in the case of an unmarried woman it might be important to the friends to know whether she had been pregnant. Intra-uterine hæmatocele is always attended by "abdominal collapse," and the bleeding goes on, except in rare instances, to death, unless the bleeding vessel be

tied, just as if the hemorrhage proceeded from the trunk of the femoral artery, no matter what the cause may have been. Extra-uterine hæmatocele may be attended by fainting, great pain, etc.; but in the great majority of cases, without any regard to the cause, the affection is without danger, and does not require any active interference. We may, if we please, try to find out whether an extra-uterine pregnancy was not at the bottom of the accident, as in the case of the extra-peritoneal variety we have the danger of the pregnancy still going on and giving any amount of trouble later. But if we have any doubt in the world, we will reserve our opinion for a few weeks, when the secret will be revealed. As regards the intra-peritoneal form attended by collapse and death, if not relieved, we may consider all cases, in the first instance, as due to ruptured tubal pregnancy, and we shall rarely be wrong. In the extra-peritoneal, the history of pregnancy may be so clear as to make the diagnosis for us at once, as soon as we ascertain that the fœtus is not inside the uterus. Or, on the other hand, the history may leave us just where it found us, or rather mislead us from the truth. Where pregnancy was possible, or likely, it will be well to await the progress of events before committing ourselves.

9. *Pelvic Inflammatory Exudations and Abscesses.* — The same remarks exactly as were made of hæmatoceles will apply here. No man can tell, in many cases, whether an inflammation or a suppuration in a broad ligament be due to a ruptured tubal pregnancy causing hæmatocele or not. Abscess of the broad ligament is the same disease, from whatever cause it may arise. And till it makes its way to the surface, and *till bones appear in the discharge*, the diagnosis is impossible—then it is self-obvious. Fortunately, as in the case of hæmatocele, it does not matter as to treatment in the very least.

10. *Cancer of the Pelvis or Peritoneum.*—This is a most important topic, as most of the glaring blunders have been in mistaking a pelvic cancer for a fœtus retained beyond full time. Strange to say, Thorburn is the only author who dwells on this point, except Tait in his recent lectures on ectopic pregnancy. Thorburn has seen four or five cases where cancer has been taken for a fœtus, even by intelligent practitioners, yet he denies that the diagnosis is difficult. It is another illustration of the general absence of correct management of the understanding, or of trusting to one symptom. Malignant tumors, as we all know, are liable to assume the most fantastic shapes; the irregu-

larities of surface, sharp projections and indentations may certainly very closely resemble a child, or the hand, foot, head, leg, arm, or ribs of a child. They imitate the shape and features of a fœtus more closely than any other known condition. But because there is a tumor like a fœtus in the abdomen, is that any reason for declaring it to be a fœtus, when an examination into the family and personal history of the patient would, in most cases, correct the fancy started by the shape? Tait tells us that a slow-growing cancer of the ovary, or behind the uterus, or elsewhere, in the neighborhood, might be difficult to diagnose, if mere pelvic examination were depended on, from extra-uterine gestation of long standing. The history, however, would here come in as a great help. The rate of growth would probably be steady, or, if a rapid period set in, the temperature would decide; for the only condition which could possibly cause a rapid increase in the size of an extra-uterine gestation sac would be suppuration; this a study of the temperature would soon reveal. As usual, the history of the case, supposing it to be an extra-uterine gestation, is not to be depended on, as it is more likely to mislead than to guide. Tait tells of a case of this kind where a man undertook an abdominal section on the supposed history of an extra-uterine gestation of long standing. In this case there was nothing distinctive about the shape of the tumor, so he had the less excuse. Upon making incision, the tumor was found to be a mass of cancer of the omentum, adhering to everything and involving everything (*op. cit.*, p. 69). This was one of those blunders which are worse than a crime as regards the reputation. The moral of all this is, never put your trust in any one mode of diagnosis, not even the most certain of all, in most cases, pelvic examination. Always have something else to corroborate any theory you may form before you let it past the stage of a working hypothesis, and above all, never *trust* the history of an extra-uterine pregnancy. Other histories, as of cancer, may be of the utmost moment, and often the history of extra-uterine pregnancy may be as clear as the day; but, again, the history may be worse than useless—the history on either side, either for extra-uterine pregnancy, or against it.

SUMMING UP ON THE DIAGNOSIS OF EXTRA-UTERINE PREGNANCY.

In the early stages it is seldom diagnosed from normal pregnancy, or from anything else, as in the great majority of cases rupture is the

first sign of *anything* being wrong. Many cases could be diagnosed were opportunity given, with great ease, but Tait is probably correct in saying that were opportunity given we should fail fifty times for once that we could be *certain.*

The cases in which the ovum survives rupture do not present any very distinctive features until the fœtal heart is heard. From that time till some little time after fœtal death, diagnosis is extremely easy. In cases where the ovum perishes at rupture, the results of rupture, extra-peritoneal hæmatocele or pelvic abscess, have absolutely no features by which they can be distinguished from cases in which the hæmatocele or abscess is due to other causes. If abscess forms and discharges, when fœtal bones appear the diagnosis is self-evident.

When, after false labor, the fœtus dies and the liquor amnii becomes absorbed, the difficulties of diagnosis begin again. And though a certain proportion of cases is almost self-evident as to diagnosis, it may justly be said that there is another and larger class where human skill and wisdom at their best can only guess. It is under such circumstances that the diagnosis from ovarian and other cysts, myoma of the uterus, pelvic cancer, etc., is likely to arise. Contrary to all authors, except Tait, I would impress on the practitioner that whatever may be trustworthy, the *history* of extra-uterine pregnancy is not so. That is to say, sometimes it may point almost without leaving room for doubt to what is the matter; but, perhaps, in a larger number of cases it only leads astray. The form of the tumor also must not be depended upon. Indeed, according to Prof. Thorburn, when the form of the tumor strongly suggests a fœtus, it is almost sure to turn out to be something else. He says that in *every case* in which he has been consulted regarding a supposed encysted fœtus the diagnosis turned out wrong (*op. cit.*, p. 438). He says (*ibid.*), that he showed one such to many experts, the late Dr. M'Clintock among the number, at the time of the meeting of the British Medical Association in Manchester in 1877, with what *appeared* to be a *perfectly typical history*, and the only difference of opinion was as to the exact position of the child. Yet it turned out to be a case of pelvi-abdominal cancer.

PART II.

TREATMENT.

As the treatment of extra-uterine pregnancy varies so much according to the period at which gestation has arrived as well as according to the state of affairs at the time the woman comes under observation, we must divide the subject somewhat as we have done the subject of diagnosis. It seems most convenient to discuss the treatment under three principal heads: First. Treatment up to time of rupture and shortly after rupture; Treatment under the various circumstances which may occur from rupture up to full term and death of the fœtus; Treatment after the death of the fœtus. Of course, these three principal divisions have to include many subdivisions.

TREATMENT OF EARLY EXTRA-UTERINE PREGNANCY, AND WHEN RUPTURE OCCURS.

Of all the revolutions in medical or surgical science, perhaps the most rapid and the most thorough has been in the treatment of ruptured extra-uterine pregnancy.

Even so recently as 1876 (when Parry wrote) the reform had not commenced. In fact, Lawson Tait's first operation proved a failure in June, 1883. Yet in five years what a change! A new operation has been established in that time. As much progress has been made in the last five years as has often taken a thousand to accomplish. Not only is the operation established, but taking the mortality from the very start it has only amounted to 4.7 per cent.! The whole thing is wonderful. To think how women have been allowed, since the earliest times, to die of hemorrhage owing to a ruptured tube, without an effort to stop the bleeding, although most strenuous efforts would have been made to have arrested almost any other bleeding. Nay, operations many times as dangerous as abdominal section would have been considered the imperative duty of any surgeon in charge of almost any other variety of hemorrhage. The principle, indeed, was often enough

asserted, but no one attempted to carry it out. In 1866 Bernutz and Goupil (p. 202) laid down the law that, "The indication in such a case is plain—we must stop the hemorrhage." But Blundell, writing in 1830, said he did not doubt that "many women die in this way, but being buried without examination, the real cause of their death is never ascertained." He even suggested the expediency of abdominal section for the control of the hemorrhage. Since his time many writers touched on the subject, but no one ventured to put the suggestion into practice until Mr. Tait (*Lancet*, October 26th, 1886; also, "Diseases of Ovaries," p. 348) broke the ice. Before 1883 the mortality was practically 100 per cent. No one can dispute the general surgical principle, "For surgical hemorrhage, cut down and tie the bleeding point," and, as Tait puts it, "If a big branch of the femoral artery were bleeding, my colleagues would cut down on and tie it. Why should Poupart's ligament be a line of demarcation within which this writ will not run?" His colleagues would tie the external iliac artery for an aneurism, why should he not tie the internal branches of the same artery? The operation comes under the category of life-saving procedures, like tracheotomy, but it has also other sufficient reasons to justify any operation, unless the mortality greatly exceeded 4.7 per cent. within the first hundred cases on record. As Professor C. A. L. Read, of Cincinnati, says, "The damaged tube, the fœtal structures and the placental tissue demand attention. Of the latter two, it may be said that, even though they become incapsuled and partially absorbed, and the residue remains innocuous, they cannot be considered, in a surgical sense, other than as foreign bodies, and as such, constant menaces to the health and life of the patient. The tube, of necessity, becomes destroyed as an oviduct, and, if left intact, it will only be to figure at a later period as a hæmato-salpinx or a pyo-salpinx, and consequently to demand extirpation."

To show the general opinion that the rupture of an extra-uterine gestation has always been considered fatal when left alone, we may quote Parry, the best and most recent writer on the subject, previous to the revolution in treatment: "From a careful examination of this subject, it must be acknowledged that a happy termination of the rupture of the cyst is exceedingly rare. Of 149 cases in which the ovum was located in that portion of the tube which does not traverse the tissues of the uterus, 145 died." Or, four cases were supposed to sur-

vive out of 149; now, out of 43 similar cases operated on, two died Indeed, no one could make out a more powerful case for operation than Parry did. He pointed out that so few recovered, that all hope of such a result should be dismissed in considering the treatment. He pointed out that Albucasis was probably the first to describe extra-uterine gestation, in the middle of the eleventh century. That, "since then men have, doubtless, watched the life ebb rapidly from the pale victim of this accident as the torrent of blood is poured into the abdominal cavity, but have never raised a hand to help her. Surely, this is an anomaly, and it *has no parallel in the whole history of human injuries.* In the whole domain of surgery—for we cannot look to other than surgical measures under the circumstances—*there is now left no field like this. In this accident, if in any, there is certain death.* The only remedy that can be proposed to rescue a woman is gastrotomy—to open the abdomen, tie the bleeding vessels, or to remove the sac entire." He thinks the first suggestion of this operation came from an American, Dr. Harbert, but to Rodgers belongs the credit of reducing the arguments to order, and advocating it. Since the time of Rodgers, laparotomy has been advocated by Meadows, Hewitt and Greenhalgh, in a discussion before the Obstetrical Society of London. Koeberle, Behier, Schroeder and Atlee countenance it, but only one performed it until 1883. Why? Parry says: "The great impediment to the adoption of this treatment is the uncertainty of diagnosis." Is this true? Decidedly not. The greatest tyro can diagnose the case sufficiently closely to tell him that it means operation or death. We have no clearer clinical entity than "abdominal collapse,"—the signs of peritoneal hemorrhage. I think, the bugbear was the fear of opening the peritoneum, that sacred ground on which the surgeon dare not trespass on pain of death to his patient and his reputation. Had it not been for the success in other abdominal operations, ovariotomy especially, I dare say we would still have been where we were in 1883. The antiseptic system, although it has had its day, will always deserve the blessings of mankind, as it gave surgeons the courage and moral support necessary to introduce abdominal surgery, and all the further advances which abdominal surgery has already led to, and will yet lead to. 1878 saw the beginning of the particular advance we are discussing, when Tait laid down his famous law, that when a patient was "in danger of death from conditions within the abdomen which do not

seem to be clearly of a malignant nature, but a correct diagnosis of which is impossible, I open the abdomen, and at once make the diagnosis certain, and a successful treatment possible." Tait points out that exploration, as a principle, is nothing new. He has seen a baronet and court surgeon remove a female breast for a tumor, and then found it was only an abscess. Here the exploration was made after the needless mutilation. Any medical man could relate dozens of such incidents, showing that complete accuracy of diagnosis is to be had in no region of the body, and that exploration is a sound, surgical principle not half enough cultivated. Tait adds, "*Absolute accuracy* of diagnosis in the abdomen is *very far* from being *possible;* only the *ignorant assert that it is,* and *only fools wait for it.*"

Some other objectors to abdominal section for ruptured extra-uterine pregnancy argue that death occurs so suddenly that in the majority of cases there is no time for operation, that the patient is too much exhausted, or moribund, before being seen. This may be true of some cases, but, as medical literature of recent date proves, it is not true of the majority. Recent medical literature swarms with cases of extra-uterine gestation; by reading these we can plainly see that, as a rule, there is time enough to obtain the services of a couple of local men, at least; and as I argued elsewhere, every country surgeon must study the matter up and prepare *his mind* to do the operation when absolutely necessary, as he has to operate for a strangulated hernia—a much more difficult and dangerous procedure. Tubal pregnancy cannot be so rare as some writers would persuade us, as between 1883 and 1888—just five years from first operation till last one published—Tait has operated on 42 cases and saved 40 lives otherwise as good as lost ("Lectures on Ectopic Pregnancy," pp. 44 and 45).

Again, at a recent meeting of the Obstetrico-Gynæcological Society of Vienna, Prof. Edward Hoffmann showed specimens of a number of cases of tubal pregnancy in which death had occurred through rupture (*Brit. Med. Jour.*, Oct. 20th, 1888, p. 903). His analysis of these cases will prove useful in many ways. He shows that: Among the eight cases, six were over 30 years of age; in one instance the age could not be determined; one of the patients was 22 years old. As to one of these women, it could be stated with certainty that she had already been confined; in another this was doubtful. In two cases the right Fallopian tube, and in six instances the left was the seat of

pregnancy. In three cases the pregnancy was situated in the middle, and in the rest of the cases in the internal portion of the tube. In half the number of the cases peritonitic adhesions of the Fallopian tube were detected. In seven cases rupture occurred in the second, and only once in the third, month of pregnancy. In one-half of the cases, several hours (*from four to twelve*) elapsed between the first symptoms of the disease and the death; in the rest of the cases, however, it was questionable whether death occurred suddenly, as there was no exact history of disease. It is thus obvious that in most of the cases, provided the diagnosis is made in proper time, there is sufficient time for surgical interference. In two cases of fresh hemorrhage the fœtus was already found in a macerated condition; this, as well as the strong torsion of the umbilical in one of these cases, led the author to conclude that the *critical hemorrhage* did not in *all instances* occur simultaneously with the *rupture of the Fallopian tube*, but *only at a later date*. That is to say, that in some of the cases the death of the ovum preceded the rupture of the tube.

Now, as I intend discussing the proper treatment in the very few cases in which a clear diagnosis is made before rupture of the tube, in some detail, I cannot do better than start with this important remark.

TREATMENT OF EXTRA-UTERINE PREGNANCY BEFORE RUPTURE, SHOULD A DIAGNOSIS BE MADE.

Of late it has been held in many quarters that the proper treatment of such a case is to kill the fœtus in some way, so as to cut the gestation short and allow the whole ovum to be absorbed.

Now, the remark that in some cases the ovum died before the rupture took place suggests a consideration of the utmost importance, at the very beginning of our inquiry. If rupture takes place after the ovum is dead, does this not seriously diminish the value of any plan of killing the fœtus? Nay more; as in intra-uterine pregnancy the death of the ovum is the great cause of abortion, may not our killing the fœtus bring on efforts at its expulsion, the false abortion which ends in rupture of the tube? In fact, may not the killing of the fœtus promote the very accident we are so anxious to guard against? Of course, it may be urged on the other side that as it is bound to end in rupture sooner or later, the sooner the better, as the ovum will be

the smaller the earlier the rupture. But do ruptures at three weeks or at two months give at all better results than those occurring at twelve or fourteen weeks? The latter question may at once be answered in the negative. Rupture at three weeks is just as certainly fatal as at three months.

In *Centralbl. für Gynäk.*, No. 15, 1888, is a case by Dr. J. Zucker, of Berlin, of an extra-uterine gestation which ruptured at the third week of its development, and where the tubal sac was "at most" not larger than a hazel-nut. No disease of the tube could be found. This is a well observed case operated on by an authority (Dr. Veit), and confirmed by a post-mortem examination, yet the rent was two inches long in the right tube, and the abdomen full of blood, four litres being removed at operation. The entire minute ovum was found; so there is not a point wanting.

Again, Dr. Parish (*New York Med. Jour.*, Oct. 20th 1883, p. 447) reports a case, also confirmed by post-mortem examination, where a woman six weeks after last menses had slight metrorrhagia; eleven days later, sudden onset of intense pain with great prostration, a mass in Douglas' pouch pushing the uterus forward against the pubes. He diagnosed ruptured tubal pregnancy with internal hemorrhage. The diagnosis was disputed. Death in thirty hours. He adds, the autopsy showed the cyst could have been removed. So here we have a case in the second month just as fatal as one in the fourteenth week. Many such cases could be cited from recent literature, and, too often, proved by autopsy, so we may consider the point proved. But what evidence is there as to the death of the fœtus producing rupture of the tube? I may confess at once that the idea is theoretical, that there is no evidence further than deductions from finding that in some cases of early rupture the ovum had been already dead for some time, and the reasoning from the parallel case of a dead ovum inside the uterus causing its own expulsion. Barnes says (*Brit. Med. Jour.*, March 10th, 1888, p. 536) that when "shreds of decidua have been extruded with uterine hemorrhage the process of abortion had fairly set in. In the case of *uterine* abortion we might safely wait, aiding nature according to her indications. But nature's course in the case of tubal gestation was to *burst the sac*, and that might be fatal. At this point rupture might be prevented by puncturing the sac, thus relieving tension by drawing off the liquor amnii and any blood which

might have been effused into it." At the same discussion the idea was mooted in a more direct way by Dr. Imlach, but he brought forward no evidence in support of his theory. He said (*ibid.*) "that opinions had advanced much since 1872, when he had seen a case at full time allowed to perish for want of surgical interference. He maintained that the same thing happened in a tubal pregnancy as did inside the uterus when the fœtus died. As soon as the child died, or was killed, the tube endeavored to discharge its contents, and, as it could not possibly pass into the uterus, the tube was ruptured. If they killed the fœtus they were doing the best they could to bring about the very thing they wished to avoid. Let it go on to full term or to rupture, and then operate at once. Laparotomy could always be performed *almost without danger*, so that there was really no excuse for killing the fœtus. He considered it to be an absurd piece of meddlesome gynæcology."

This is all the evidence I can find in favor of the idea that killing the fœtus may cause rupture of the tube sooner than it would naturally occur; but as I am not prepared to throw overboard wholesale all the evidence as to ova killed by puncture, electricity, etc., so I cannot place theory against facts, and I therefore abandon the idea as non-proven. I think it worth further investigation, however, as there may be peculiar circumstances under which death of the fœtus will provoke rupture of the tube. There are many other objections to all plans of killing the fœtus, such as those of Tait: that it is immoral, that the fœtus has a right to its chance of life, that the placenta may grow after fœtal death, that death of the fœtus does not free the woman from the many dangers which encompass her, that there is neither certainty nor safety in any of the methods employed to kill the fœtus, that most of them—all those involving any puncture—are actually more dangerous than an abdominal section, and that they will commend themselves only to those who lack courage and skill to obtain good results by abdominal section. The last objection is, I think, the principal reason for further research to determine whether there is any good in killing the fœtus. We cannot have a Lawson Tait within reach of every poor woman in every small town or country village, and if there is any easier way of relieving her of the danger which an extra-uterine pregnancy involves, it would be well that all should know of and be familiar with it. We will discuss Tait's objections

presently, and try to ascertain what truth there may be in them; but, first, I would remark that most of his objections only apply properly to killing the fœtus at a rather advanced period of pregnancy, whereas these methods are intended specially for the pre-rupture stage. But, unfortunately, no matter what treatment may be discovered for the pre-rupture stage, it could only be of the most limited advantage, as in the vast majority of cases the rupture itself is the first warning of anything being amiss.

To take Tait's objections in order: I will not waste words on the objection that killing the fœtus is immoral, as in the stage of which we are considering the treatment the chances of life for the fœtus are so poor that they cannot be much worse. The great majority die at rupture (with the mother or by the operation to save her life). Then of the cases where the rupture is into the broad ligament (calculated at one-fourth by Tait), the great majority of this minority die either at the period of rupture or, at all events, before the viable age. So that, as Tait himself says, it is only the minority of the minority which reaches the viable age. Then, suppose we could nurse an extra-uterine pregnancy to the viable period, would it really be so very moral and praiseworthy, if we could terminate it safely, to continue a pregnancy so fraught with danger to the mother? I certainly do not see the immorality of killing the ovum before rupture. Next, as to the danger of the placenta continuing to grow after fœtal death. At the debate at the British Gynæcological Society, Feb. 8th, 1888. where Tait made this assertion, Dr. Inglis Parsons said that there was no evidence to show that the placenta would continue to grow when the child had been killed by electricity. The force that could produce the death of the fœtus was not likely to leave the placenta untouched. No subsequent trouble had resulted in the twelve cases mentioned by Dr. Garrigues. (The paper of Dr. Garrigues is to be found in the "Transactions of the American Gynæcological Society" for the year 1882.) Now, I do not know that there is any evidence of growth of placenta after the killing of the child *by electricity.* But if Dr. Parsons meant that there was no evidence of the growth of the placenta after fœtal death generally, he was mistaken. Unless we are prepared to deny the truth of reports by Tait, he has proved it over and over again. In his "Lectures on Ectopic Gestation," 1888, pp. 73, 74 and 75, quite a number of cases are reported. He says it is a

frequent experience to find a large placenta embracing a small ovum cavity without any, or with only very slight, traces of a fœtus in the so-called uterine "moles." If this be so, we have no reason to do other than expect that the same thing will occur occasionally in tubal pregnancy. But Tait is not alone in this belief. Dr. Champneys and Mr. Thornton gave evidence to the same effect before the London Obstetrical Society, and Hart and Barbour, in their "Manual of Gynæcology," 1882, have the following:—

"Case of extra-uterine gestation, with death of the fœtus, but *continued growth of the placenta*, which led to fatal hemorrhage." A. B., æt. 24, had passed two periods without menstruating; three months ago she began to have irregular hemorrhages three times a month, and in considerable quantity. The tumor was found in the pelvis, the vagina being compressed against the pubis, the cervix reaching above the brim, and the bladder displaced into the abdomen. The tumor was as large as a uterine pregnancy of 4½ months. After a puncture of the cyst with the aspirating needle, the patient died, with symptoms of internal hemorrhage, and on a post-mortem examination, by freezing the pelvis and cutting sections, the uterus was found to be 5¾ inches long, the fundus being five inches above the symphysis, and the cervix so drawn up that the fornices are obliterated. The gestation sac lay in the pouch of Douglas, and was chiefly occupied by the placenta. The cavity of the amnion contained but little fluid, and the fœtus was about the size of a three months' pregnancy. The continued growth of the placenta after the death of the fœtus had led to fatal hemorrhage.

We have thus evidence enough of the growth of the placenta after fœtal death. But Tait himself admits when describing the way he now deals with the large placenta found in operations at full time or after, viz., cuts off the cord close and hermetically seals up the wound, that the placenta, as a rule, is the first of the tissues to disappear by absorption, notwithstanding the numerous cases in which it grows for a time after fœtal death ("Lectures," p. 97). Still, if the placenta grew for a time after the child was killed in the pre-rupture stage, it would be enough to cause rupture of the tube. The next objection to killing the fœtus is that it does not save the woman from the many dangers which encompass her. This is such a good argument in the advanced stages of pregnancy that I wonder any one persists in the killing plan

after, say, seven months. We find that in the vast majority nature kills the child, yet is the mother any better except once in a hundred cases? The child dies naturally in all cases at some time or other, yet we have serious troubles after that. Indeed, when the fœtus dies some time after rupture, inflammation and suppuration in the sac usually take place followed by elimination of the fœtus as a foreign body. It is a very rare case, indeed, where this does not happen sooner or later.

But I do not think this argument would have much weight against killing the fœtus before rupture, or even shortly afterward, as we know that should the fœtus perish shortly after rupture, as a rule, fœtus and blood are alike absorbed, as in simple hæmatocele, so much so, indeed, that it is impossible to tell a hæmatocele from ruptured tubal pregnancy from one due to other causes. The next argument against the various methods of killing the fœtus is that there is neither certainty nor safety in any of them. This is confessed by their advocates. There is ample proof of this proposition. The horrible case of Matthews Duncan, in the Bartholomew's Hospital Reports for 1883 proves it by itself. In this case the induced current as strong as the faradic coil of a Coxeter's combined battery could give was used first. The positive pole placed over the abdomen and an insulated negative pole in the vagina. The current was passed and withheld during spaces of two seconds for a minute and a half. Then the continuous current from forty modified Leclanché cells was passed for six minutes, producing slight blistering of the skin and a dried surface in the vagina. Heart sounds audible. Next day two grains of morphine were injected into the amniotic cavity. Then the liquor amnii was drawn off through the abdominal wall by the aspirator; eight ounces were removed. Heart sounds still audible. Five days later, $\frac{1}{4}$ grain morphine was injected two inches deep into the body of the fœtus at the place the heart was best heard. This was twice repeated at intervals of two days without any result. Galvano-puncture was now used. Two insulated electrolysis needles were passed an inch and a half into the tumor and connected with the negative pole; positive pole to abdomen. A current from forty Leclanché cells was passed for six minutes with some interruptions. After all the fœtal heart was beating, but more slowly. The fœtal heart still being audible four days after all this, the tumor was aspirated again, and thirteen minims of equal parts of

water and a hypodermic solution of morphine injected again into the body of fœtus over the heart. Fœtal heart ceased at last. The patient died two days afterward, which no doubt was not surprising to the great gynæcologist who conducted this horrible series of experiments. At the autopsy, twenty-six hours after death, the contents of the cyst were very fetid, the soft parts of the fœtus completely macerated; the bones exposed. The fœtal internal organs were diffused in the fetid fluid or were semi-fluid. Such a case is a serious impeachment of all the modes of killing the fœtus. Here they were all combined to a sickening and fatal extent. The child was only killed two days in advance of the mother, whereas, as Tait points out, both might have been saved by following the ordinary principles of surgery. A few more such victories as this and all these methods would be lost. If this case had occurred since Tait demonstrated the comparative safety of laparotomy, we would consider this case of Matthews Duncan a disgrace to the medical profession; but even in 1883 the peritoneum was still a sacred ground from the knife, though not at all sacred from the needle and the trocar, which are, at least, quite as dangerous. The last sentence contains the next argument against fœtus killing. The trocar or aspirator in the case of abdominal diseases may tell that the tumor contains serum, blood or pus, but how much is the information worth? Then as to treatment, for what condition could the aspirator be considered final and sufficient treatment? Hear Parry on the use of the trocar in extra-uterine pregnancy. "In cases of doubt, the fœtus being dead, the trocar has been used to draw off some liquor amnii in order to confirm the diagnosis. *Unless it has been decided to operate immediately* for the removal of the fœtus, the use of the trocar is utterly unjustifiable. A few, but *very few, women have long survived its use.* Jonathan Hutchinson has had fatal peritonitis after its use. He says the practice "is itself attended by great danger; nor shall I deal honestly with you or myself if I do not candidly admit that, with due care and patience, I do not think that paracentesis ought to be necessary in a case of fœtal tumor simulating ovarian dropsy." We have now reached the end of Tait's objections to the process of fœtus killing, and will next inquire into the various plans proposed, a little in detail.

METHODS WHICH HAVE BEEN RECOMMENDED FOR BRINGING THE PREGNANCY TO AN END IN THE PRE-RUPTURE STAGE.

Puncture of the Sac.—This is said to be easily effected by the introduction of an exploring trocar through the vaginal or rectal wall. The aspirator needle has also been employed. But supposing the diagnosis made, it might not be so easy as it looks to put the needle into a freely movable tumor, or, at least, to put it into such a position that it would draw off the liquor amnii. Then suppose the liquor amnii drawn off, is the fœtus sure to die? We have the authority of Dr. G. Ernest Herman, Obstetric Physician to London Hospital, for it, that he has known it to be drawn off in cases of five or six months' pregnancy without killing the fœtus, and there seems no reason why it should be more fatal in the early stage. But if harmless to the fœtus, it can hardly be said to be harmless to the mother, and it has not been attended with very brilliant results in any sense. Parry says mothers rarely survive it long, but recoveries have been recorded by Tanner, Stoltz, Jacobi, Koeberlé, Greenhalgh, and E. Martin (two cases). However, death from peritonitis and septicæmia have followed this "simple" procedure in the hands of Sir J. Y. Simpson, Martin, A. Simpson, Braxton Hicks, Thomas (two cases), Conrad, Routh, Netzel, Hutchinson, John Scott, Gallard, and Depaul. Fränkel ("Zur Diagnostik und operativen Behandlung der Tubenschwangerschaft," *Arch. f. Gynäk.*, Bd. XIV, p. 197) drew off nearly three-fifths of an ounce of liquor amnii without killing the fœtus. Then the blood vessels in early extra-uterine gestation sacs enter from below, so that they are in danger of being wounded, and recent literature shows many cases of hemorrhage following tapping. In the more advanced stages of pregnancy the danger of tapping is very great. In the case of Dr. Braxton Hicks, the patient died, when four months pregnant, of internal hemorrhage, the result of an attempt to kill the child by puncture. In *Brit. Med. Jour.*, Dec. 1st, 1888, is a case to which I have already referred for another point. The patient recovered after laparotomy at the fourth month. As the urine gave half its bulk of albumin, it was urged at the consultation that under the circumstances it would be wiser to tap. The operation showed that tapping would have been fatal through hemorrhage. If the patient had by a chance survived, she would have been rendered so much the worse by the complication of an intra-peritoneal hæmatocele. But modern experi-

ence seems to decide that this condition is always fatal by repeated hemorrhages, if not the first time, so that the woman would have been practically killed by this "simple" and "harmless" operation. The truth is that people without the skill or courage to do an operation will have the audacity to plunge a trocar in anywhere, because it is not usually attended with immediately fatal results. However, in Jordan's case, such was very nearly the result, as two hours after the tapping "complete collapse came on" (Tait's "Lectures," p. 69). Therefore, puncture is, as Tait maintains, quite as dangerous as abdominal section. The result is at least uncertain; indeed, it has never been properly proved to be efficient. Certainly cases are recorded in which a pelvic tumor has been tapped, some serum has been drawn off, the tumors have subsided, but anything like positive proof that these tumors were extra-uterine pregnancies is wanting. Such is the proceeding which, even in the present year (*Brit. Med. Jour.*, March 10th, 1888, p. 536), Dr. Robert Barnes, "with the present state of knowledge, prefers to *all others*," reserving, of course, the right to proceed to ulterior measures, according to circumstances. He distinctly says: "In the early stage, before bursting of the sac, electricity to kill the fœtus might be applicable; but simple puncture of the sac was better." Dr. Routh (*ibid.*) agreed with Dr. Barnes. If electricity is really worse still than simple puncture, I should say it will not remain long in fashion. Barnes declares it is probable that the puncture which forms a part of the other methods, such as electricity and morphine injections, is by itself adequate to account for the successful results. If so, so much the worse for the other methods. Playfair joins in the opinion that the good results of the other plans would follow on simple puncture. One of the arguments used in favor of simple puncture by aspirator is that, in case of a mistake in diagnosis, it would do no harm. This is very different from Parry's view that few women survived it (*op. cit.*, p. 204).

Injections of Solutions into the Sac.—Joulin ("Traité complet des accouchements," p. 968) proposed that, in addition to puncture, we should inject something into the sac which would destroy the fœtus. He must have had his doubts about the efficiency of simple puncture. He proposed injections of sulphate of atropia, one-fifth of a grain dissolved in a few minims of water, by means of a hypodermic syringe, with an extra long needle. His idea was carried out successfully by

Friederich of Heidelberg (Cohnstein, "Beitrag zur Schwangerschaft ausserhalb der Gebärmutter," *Arch. für Gynäk.*, Bd. xiv, p. 355). He recommended that the needle of the syringe should be put into the sac through the abdominal or vaginal walls, a few drops of fluid first withdrawn and then the poison injected. Friederich preferred a fifth of a grain of morphine. He repeated the dose every second day, until the diminished size of the sac gave proof that the fœtus had been destroyed. This he did in two cases. There was but slight inflammatory reaction, and the woman did not seem affected by the morphine.

Rennert ("Extrauterin-Schwangerschaft im fünften Monate," *Arch. für Gynäk.*, vol. xxiv, p. 266) afterward killed a fœtus in the fifth month of extra-uterine pregnancy by one injection of half a grain of morphine. The patient had a severe illness, but finally recovered. In the Gynæcological Section of the Eighth International Medical Congress at Copenhagen, Koeberlé related a case of advanced extra-uterine pregnancy where morphine was successful. The woman made a complete recovery, the placenta and child being both completely absorbed. But Hennig reports also a case operated on by Koeberlé, where profuse hemorrhage occurred. He does not state whether the case was fatal. ("Die Krankheiten der Eileiter und die Tubenschwangerschaft," p. 138.)

The slight successes scored by this method of injecting poisons into the gestation sac, are surely more than counterbalanced by the horrible case of Dr. Matthews Duncan, already referred to. Any one who is at all inclined to favor this mode of treatment, should read the full account of the case which is to be found in "St. Bartholomew's Hospital Reports," vol. xix, 1883, p. 35. My opinion is that this one case is quite enough to discredit the morphine treatment altogether. It was by accident more thoroughly tried than the experimenter wished, as, on one occasion, two grains of morphine were injected in mistake for one-fifth of a grain. The only perceptible effect of this large dose was on the mother, whose pupils contracted and who felt drowsy.

Elytrotomy.—Thomas's operation of cutting from the *vagina with the cautery knife.*

Professor Gaillard Thomas relates a case where he cut into the sac with the red-hot knife attached to the electric-cautery. He very nearly lost the patient, but she finally recovered.

Lately, he recommends Paquelin's cautery at a red heat. After slowly cutting the sac, he recommends that the fœtus should be removed, the placenta being suffered to remain. The sac is then to be filled with antiseptic cotton, and this renewed once in thirty-six hours. He recommends the operation only where instant action is required, owing to urgency of symptoms. I do not see on what possible grounds this operation could be preferred to simple abdominal section. It seems much more difficult, and could not possibly give better results. It has been spoken of by an authority as "a bold and judicious proceeding." But I quite agree with Dr. Herman (*Lancet*, May 26th, 1888, p. 1022), in thinking it "bold, but not judicious." As Herman remarks, the operator cannot tell into what tissue he is pushing his incandescent knife, and, should the diagnosis be erroneous, the operation would be likely to prove fatal. Dr. Herman ("Delivery by the Vagina in Extra-uterine Gestation," "Trans. Obstetrical Society, London," 1887) has collected the cases of this operation, and criticises it very severely. His conclusions are fatal to the operation at any stage of pregnancy, but here we are only concerned with it previous to rupture. He characterizes the opening of an extra-uterine gestation sac from the vagina early in pregnancy, before rupture has taken place, by the cautery knife or otherwise, as "a dangerous and unscientific" proceeding. Abdominal section should always be preferred. It seems a mere vagary of one in search of novelty or notoriety. Tait is equally opposed to it.

Electricity.—This agent, in various forms, has been used to kill the fœtus in extra-uterine pregnancy since the middle of the present century. The suggestion came from France. Moreau states that Voillemier was the man who first proposed it. Cazeaux also made the suggestion ("Midwifery," 1850) by inquiring whether the death of the fœtus were not possible by passing an electric shock through the uterus. Bachetti, an Italian, was the first to actually make trial of the suggestion. He introduced two needles into the tumor and connected them with the wires of an electro-magnetic machine, on February 2d, 1853. He reported pregnancy arrested after two shocks. This was a case of galvano-puncture. Dr. Braxton Hicks was the first to try electricity minus the puncture. In 1866 he applied one pole to the abdominal prominence of the tumor and the other in the vagina. He abandoned the plan after two trials, resorted to the favorite puncture

from the vagina, and killed his patient. Dr. J. G. Allen, of Philadelphia, was the first to employ electricity, minus the fatal puncture, with success. His first case was in 1869, three years after Braxton Hicks had failed by the exact same method. Dr. Allen had another successful case in 1871. Electro-puncture had succeeded in shriveling up the ovum in a case of tubal gestation in the hands of Burci in 1859. He used two acupuncture needles. Dr. Allen did not report his successes. It seems the story transpired incidentally, at a debate before the Obstetrical Society at Philadelphia. As in the case of so many inventions, it was forgotten for a time, or, at least, no more was heard of it until, in 1877, Drs. Landis and Lovering had a successful case. Since 1877 the practice has become quite a favorite among American practitioners, and it may, at once, be admitted to be a vast improvement on the puncture in any of its shapes or forms. It would be very hazardous to say that in case of error of diagnosis, electricity, even minus the puncture, would be harmless; but, at least, it would be comparatively so, as compared with the puncture. Quite a number of successful cases are now on record, principally by Americans, and in no small measure due to the enthusiasm of Prof. T. G. Thomas, of New York. Thomas saw most of the cases in consultation, and urged a trial of electricity in the form of the faradic current. Dr. J. C. Reeve ("Trans. of the Amer. Gynæc. Soc.," vol. iv, p. 313), Dr. H. P. C. Wilson (*Amer. Jour. of Obstet.*, vol. xiii, p. 836), Drs. Harrison, Herrick, Cushier, Cocks, Lambert, P. F. Mundé, and Garrigues, have each reported one successful case, Landis a second one, and W. T. Lusk two. In six cases treated, one by Dr. McBurney ("Case of Tubo-interstitial Pregnancy," *New York Med. Jour.*, vol. xxvii, p. 273), one by Dr. C. E. Billington, two by Rockwell, one by Mundé, and one by Dr. Herrick, equal success was attained by the employment of the galvanic currents with interruptions (120 to the minute). In McBurney's case two applications, in Billington's four, and in Rockwell's one only, were required. Drs. Thomas and Garrigues prefer the faradic current, as they consider it equally successful, and not so unpleasant to the patient. Details of most of these cases will be found in the two papers of Dr. Thomas in the "Trans of the Amer. Gynæc. Soc.," 1882 and 1884, and in the paper of Dr. Garrigues, *ibid.*, 1882. In Prof. Lusk's first case the faradic current was applied at the end of the tenth week from last menses. The sac was very low

down on the right of the vagina; ballottement of the entire ovum could be obtained by the bi-manual method, and fluctuation was plainly felt. Thomas confirmed the diagnosis. For the first three applications no change was observed, but at the tenth distinct changes were found. Treatment was then abandoned and the tumor almost disappeared. Fifteen successful cases in all had been published up to the autumn of 1886. The Americans have only used electricity during the first four months of extra-uterine pregnancy. According to Dr. Lusk (*Brit. Med. Jour.*, Dec. 4th, 1886, p. 1094), the galvanic current usually kills the fœtus at a single séance, but it must be used of great strength. The faradic current might be used, of moderate intensity. It should be applied for five minutes at a time, twice daily, for several days, until distinct shrinkage of the tumor is detected. The most convenient and successful method consisted in placing the negative pole over the tumor through the rectum, and the positive pole over the abdomen.

Dr. Landis, in a recent communication (*American Jour. of Med. Sciences*, Oct., 1885) arrives at the following conclusions after conducting a series of experiments based upon the supposition that the fœtus can be killed if subjected to a current strong enough: 1. In using the faradic current in extra-uterine pregnancy, the applications ought to last an hour, if the patient can bear it. 2. The current should be repeatedly applied, in order that the vitality of the fœtus may be completely exhausted. 3. For at least one séance the current should be used in great strength. 4. The current probably acts upon the placental circulation, as well as by destroying the fœtus.

Duchenne, Barnes and others have suggested the use of a spark of Franklinic electricity in the form of a shock from the Leyden jar, but the galvanic has been preferred, because by a more prolonged application it was hoped that the chemical effects of the current would aid the shock. In fact, it was hoped by the electrolytic action to obtain results beyond the mere death of the fœtus. That is, it was hoped that the chemical action, of which we have many examples in the coagulation and decomposition of albuminous tissues and its power of splitting up both organic and inorganic substances into various absorbable products, would aid in the absorption of the dead fœtus, the placenta and sac. We have a striking example of this electrolytic action in the recent application of electricity for the decomposition and subsequent absorp-

tion of cicatricial tissue in the urethra. This hope has received a certain amount of confirmation in the famous case of Dr. R. Petch (*Brit. Med. Jour.*, Dec. 4th, 1886, p. 1092) where a six months' fœtus was killed and completely absorbed. But then he inserted two needles into the body of the fœtus, so that the treatment was galvano-puncture, as distinguished from the American mode minus the puncture. Then it is very hard to say what nature can, and what she cannot do. We know that many ova are absorbed entirely at four or five months without any treatment, and that the placenta can be absorbed even when of full growth. Leopold (*Arch. für Gyn.*, Band xviii, S. 53) experimented on animals by opening the uterus and turning the embryos out into the peritoneum, and he found they were quite absorbed. Even the liquor amnii can absorb a very young embryo. Smellie ("Midwifery," New Syd. Soc., vol. ii, p. 68) relates a case in which the amnion was expelled separately from the chorion and the fœtus visibly inside it. After twelve hours most of the fœtus was dissolved. Then Tait in forty cases found the placenta every time, but only twelve fœtuses. Certainly when an extra-uterine fœtus dies about the fifth or sixth month suppuration, as a rule, takes place in the sac, sooner or later, and the fœtal bones are eliminated; and even when the sac does not suppurate the fœtus may be retained for many years without either bones or flesh being absorbed. So that, as far as we dare draw conclusions from one case, the case of Dr. Petch seems in favor of the power of electricity to favor absorption of the fœtus. Then we have Apostoli and Doléris recommending the treatment of circum-uterine hæmatocele by negative galvano-puncture—*puncture again.* According to these authorities it rapidly cures. They say its action is twofold; surgical, by creating a fistula in the solid effusions, the neoplasms, the walls of cysts, etc., and thus effecting a permanent means of elimination; and medical, by provoking disintegration of the tissues, followed by rapid and manifest resolution (*The London Medical Record*, Feb. 15th, 1886, p. 77). But Apostoli seems to explain the wonderful results he claims for electricity in peri-uterine inflammation, fibroids, etc., more by the sinus effects of the puncture than by electrolytic action (*Brit. Med. Jour.*, Nov. 19th, 1887, p. 1096). Again, he confesses in regard to myomata that his method "was in no sense a radical cure, but that it would, under favorable circumstances,

lead to an amelioration of the most distressing symptoms." (*London Medical Recorder*, Aug. 20th, 1888, p. 322.)

It should also be remembered that shortly after M. Apostoli had astonished the world by his revival of electric treatment, he suddenly improved upon his modus operandi, and instead of merely passing currents through the uterus and its tumors by means of one electrode in the vagina or uterus, and the other applied to the abdominal walls, arrangements were made for plunging insulated needles into the substance of the tumors. The action set up in this way was ascribed to electrolysis, that is, electrical decomposition of the tissues into their component parts. In the opinion of Alfred S. Gubb (*Lond. Med. Recorder*, Aug. 20th, 1888, p. 322) the idea of electrolytic action was probably erroneous. He says, so far, no trustworthy evidence has been adduced of the existence of any such action; and that it is more than probable that the success of the treatment is due to the purely caustic action of the current, as it is well known that if the degenerative process be once started in these tumors it is very apt to go on until the tumor is destroyed.

In one of the most recent cases of the employment of electricity in extra-uterine pregnancy, reported by Dr. Buckmaster, of Brooklyn (*Medical News*, July 21st, 1888), Lawson Tait seems, to my mind, to have proved that the whole case is inconclusive, as he says most of these cases are. The case is too long to discuss here, but Tait's remarks will be found at pp. 71, 72, 73, and 74 of his "Lectures on Ectopic Pregnancy."

We have not had much experience of the electric treatment of extra-uterine pregnancy in England. The famous case of Dr. Petch was the first successful one, but galvano-puncture was employed. It was done on August 20th, 1878. The first successful case of the American plan, without puncture, was that of Dr. James H. Aveling, senior physician to the Chelsea Hospital for Women, done on May 10th, 1881. The case was in the pre-rupture stage, and his diagnosis was confirmed by Sir Spencer Wells. Gaiffe's induction apparatus was employed, with two cells, but not more than half its power was used. The negative pole was applied to the swelling through the vagina, the positive pole being placed on the abdominal wall. It was applied for ten minutes on four days, when the tumor was found

smaller, the pulsation had ceased, the attacks of pain were no longer felt, and in about two and one-half months only a small, flat, elongated body could be detected in which there was no tenderness. The pregnancy was between the second and third month when treatment commenced. The current caused no actual pain. He believes the fœtus died by shock, as there could be little or no chemical action in the electricity from a Gaiffe's induction machine. He believes electricity is the only safe and justifiable method of fœticide during the first four months of extra-uterine pregnancy (*Brit. Med. Jour.*, Dec. 4th, 1886, p. 1091). Now, regarding the electric treatment of extra-uterine pregnancy, I think any fair-minded man must admit that the greater part of the literature of the subject is far from convincing. As Herman says, "There are many cases in the papers of Thomas and Garrigues in which the diagnosis rests on such slender grounds that it is impossible not to suspect error. The strength of a chain is in its weakest link, and when one finds seriously adduced as *scientific evidence* cases of an extremely doubtful kind, without, apparently, a sufficient sense of the possibility of mistake, one is apt to judge of the strength of the whole chain of evidence by these weak links." Of course, no one doubts Petch's case, but even here, where there was only one application, neither fœtal heart sounds nor movements were influenced by the current at the time, the next day they were weaker, and could not be heard on the fourth day. It is not uncommon for the fœtus to die at any time during extra-uterine gestation, and besides, there was the puncture, which Barnes says is the active agent. The conclusion is that we require more cases, and far more conclusive cases as to diagnosis, before we can admit it as proved that the galvanic or faradic current can kill the fœtus. Then there is an accurate enough case against the idea, the horrible one of Matthews Duncan. One case like this weighs more than many such as that of Dr. Aveling. Indeed, the case of Matthews Duncan gives great support to the opinion of that eminent electrician, Dr. Steavenson, who has had so much experience in treating urethral strictures by electricity. Steavenson admits that *galvanic* shocks, when the puncture is used, can kill the fœtus, but he questions whether *faradic* currents without puncture can. His reasons are—1st. That the inhibitory apparatus of the heart is not developed in a fœtus three or four months old; 2d. That the tenacity of life in a fœtus is comparable to that of the lower animals, such as tadpoles, and

that any current that was capable of destroying the life of the *fœtus* was *much more likely* to destroy the life of the *mother*. Tait says he cannot accept the twenty cases collected by Dr. Aveling, which disappeared under electricity, as cases of tubal pregnancy, and disbelieve his own observations. Besides, he believed, if such cases went on to the viable period *all the children* and *most of the mothers* might be *saved* by proper operation at the proper time. At the close of the latest debate on the subject before the British Gynæcological Society, on February 22d, 1888, the President, Dr. Arthur Edis, said the facts before us did not justify a clear expression of opinion. He would not recommend any one for whom he had a regard, and who was suffering from an extra-uterine gestation, to be electrified, but would prefer opening the abdomen and removing the mass at once. The cases in which dead fœtuses had been left for long periods of time inside the body without giving rise to symptoms were very exceptional, and the electrical treatment might be the means of causing the very mischief they were endeavoring to avoid.

Then as to the alleged harmlessness of the electric current in case of mistaken diagnosis, there have been deaths accredited to electrical treatment of abdominal tumors. From the dangers we have shown to follow the simple puncture, it follows that galvano-puncture cannot be without very serious dangers. But even the electric current without the puncture, according to an American, Dr. Baldy, is not without serious risks. At all events, serious accidents have followed its use in an alarming number of instances, considering how few cases have been so treated. In the *Obstetrical Gazette*, June, 1888, will be found the most interesting and instructive article of Dr. Baldy. "He regrets that the majority of American surgeons favor the electrical treatment of extra-uterine gestations. He thinks after rupture there can be no two opinions about abdominal section being the *only treatment*. He believes the electrical treatment will undoubtedly kill the fœtus, and that it has advantages, such as doing away with the so-called mutilation of the woman and with the worry and excitement of an operation. There are, however, disadvantages which cannot be overlooked. There are cases on record in which the *fœtus has ulcerated its way out*. Dr. Mann states that there are *only two* such cases on record, and that, as the number is so small, they need attach no great importance to them, but, if these two cases are compared with the total number of

cases of extra-uterine pregnancy treated in this way and reported within the last few years, the proportion is decidedly large. Then, there have been *four cases* reported of rupture of a blood vessel without rupture of the cyst. In one case, that of Dr. Janevin, the vessel ruptured some days after the fœtus had been killed by electricity, and the *patient lost her life.* We might as well attribute this ruptured blood vessel to the electric current, as Dr. Petch may the death of the fœtus in his case, which occurred four days afterward. Death from rupture of a vessel was a positive danger, and one which could not be ignored. As far as Dr. Baldy knew, there had never been a death which could be attributed to the knife. A number had died after the operation, but they had died in consequence of the rupture, and not from the operation. The fact that the fœtus had been killed (assuming for the sake of argument that this really took place) did not render the Fallopian tube pervious, but leaves the woman sterile from that side as effectually as if it had been removed by the knife and ligature. Dr. Lusk had reported two cases, in one of which, *after six years,* there was a *mass still remaining* as large as a pigeon's egg, and in the other, after *four years,* the tumor is as large as a hazel nut. This was the usual history of these cases. He was in favor of removing any pathological growth whatever from any portion of the body, at any time, if it can be done without risk to life. The masses which were allowed to remain were as likely to set up subsequent trouble as some of the tubal and ovarian troubles for which they operated every day. It seemed to him that the gentlemen who *accept the knife* for the *least dangerous,* and refuse it for the more dangerous, troubles are extremely inconsistent, to say the least of it. By operation the cyst is removed, the fear of subsequent danger is done away with, and the risk of the continued growth of the placenta, however small that risk may be, is also avoided."

It seems to me that Dr. Baldy has quite snuffed out the "electrolysis" party—that is, if they needed any snuffing out. But the electrical treatment was a noble and ingenious effort to do something for a class of cases for which medical science at the time could do nothing. As Parry said, it was an anomaly in surgery, that for these cases of bleeding to death from ruptured tubal pregnancy there was no hope and no help. But, before long, every surgeon will see that Tait's discovery that the bleeding can be stopped and the whole present and

prospective danger removed by an operation which at the outset is attended with a mortality of only 4.7 per cent. supersedes the electric discovery and every other uncertain, unsafe, and elaborate plan of treatment. People, as yet, can hardly realize the fact that an accident, which since the earliest ages was looked on as a fatal accident, can be remedied by such a very safe procedure as abdominal incision. When the fact is realized, all other plans of treatment will speedily die away and be heard of no more. Had Tait's discovery not taken place I would have said the electrolysis idea should be pushed and developed to the utmost; but now, it is really not worth while. Any procedure with any danger at all attached to it—and what operation has not?—would probably have as high a mortality as Tait's operation, and no other could be so certain, so thorough, and so free from after-dangers. I quite believe, with Tait, that tapping for ascites, or for any other purpose, is quite as dangerous, if not more so, than abdominal incision. Yet the most timid and ignorant country doctor will thrust a trocar into the abdomen without one uneasy thought. But we must allow time for the facts to be realized. Most of the people on whom Tait operated were already nearly dead with shock and hemorrhage, and the two he lost out of forty-three, no doubt, as Dr. Baldy says, died not from the incision but from the shock they already suffered from. I dare say, if Tait could operate on a number of extra-uterine gestations previous to rupture, the mortality would be about *nil;* but then he says he can't get hold of them till rupture occurs, as they don't feel much the matter; and really this objection is *fatal* to the electrical treatment also. I would say, then, that the proper treatment of extra-uterine gestation in the pre-rupture stage, whenever diagnosed, or suspected with great probability rather, is instant abdominal incision and removal of the whole trouble.

TREATMENT OF EXTRA-UTERINE PREGNANCY AT PERIOD OF PRIMARY RUPTURE.

Every authority is agreed as to the only treatment of any value once rupture has occurred. We have discussed this so fully in other parts of the essay that it only remains to describe the operation and the various matters connected with it. We need not touch on the symptoms, as they have been fully considered under diagnosis. We shall first look at the history of the operation.

History.—Heister, in giving the indications for abdominal section, places extra-uterine gestation first, "when the fœtus is contained in the Fallopian tube, the ovary, or the cavity of the abdomen." Simon ("Mem. de l'Acad. de Chirurg.," Paris, vol. ii) speaks of the same indication in his account of Cæsarean section. It is difficult to say when the first real laparotomy was done, as the very early operations were probably only done when suppuration had taken place in the gestation sac and the pelvic abscess was in process of pointing in some direction. The earliest of which we have any account was in 1540, when Christopher Bain operated, but the probabilities are that this operation was only the opening of a broad ligament abscess. The same may be said of the next operations, those of Noierus in 1591, and of Cyprian in 1694. The operation of Primerose, in October, 1594, was a true laparotomy. His patient had two extra-uterine pregnancies. The first time in 1591, when Jacob Noierus extracted the child, as already mentioned, when the pelvic abscess containing it had already burst, that is, he enlarged the fistula. The second time she was pregnant, Primerose removed the child by a real abdominal section. Three years later we have the report by Felix Platerus of another successful case. Then, for more than a hundred years there is no mention of a case. In 1714, a case was operated upon in France, reported by Calvo. The next case there is any record of was one by John Bard, of New York, who operated in 1764—the first time in America. The next operation was also done in America, and was successful. It took place on January 14th, 1791, the operator being Dr. Wm. Baynham. Baynham had another success on February 6th, 1799. Another American case, by a Mr. Knight, was published in 1795. After these four cases in America a quarter of a century elapsed before the operation was repeated by Dr. Wishart, on the 6th of October, 1823. Dr. A. H. Stevens, of New York, did the next, on February 6th, 1846. Except the last, the other five operations in America were done by country doctors, and the country doctors will have to take heart and do the operation again, as they well could. About 1850 ovariotomy commenced, but in 1876 Parry could only collect 62 operations for extra-uterine gestation. Even this, however, the first list ever made, showed so much success that it is wonderful the operation was never done as a life-saving procedure during the collapse of rupture. In Parry's list of 62 there were 30 successes to

32 failures. Dr. Harbert, also an American, was the first to definitely suggest the opening of the abdomen for rupture, in 1849, and Dr. Stephen Rogers, of New York, also urged it in 1866, '67. It is very strange how America has been to the front in connection with this subject, and it is to be hoped that Americans will be more than ever connected with it, in carrying out the life-saving operation first urged by Americans. Nobody, however, found courage to carry out the procedure which was indicated by the ordinary principles of surgery until in June, 1883, Lawson Tait commenced a new era in surgery. Although his first operation was a failure, in five years from that date he has placed the operation in the front rank of procedures for the direct rescue of human lives from immediate death.

Extra-uterine gestation has hitherto, and with good reason, been considered as one of the most fatal of diseases.

About three-fourths of all cases die, and more than half die shortly after the primary rupture.

In considering the treatment of a ruptured tubal pregnancy, the great point to remember is that it is, so far as the tendency to death goes, a "concealed hemorrhage," and that every moment is of value. The patient may not die for many hours, but every hour that operation is postponed diminishes her chances of recovery, even when the operation is at last done. The blood tends to keep oozing into the peritoneal cavity all the time, with gushes at intervals. This is owing to the peculiarities of the condition of affairs as before explained. Owing to the free space for distention in the peritoneal cavity, and the fact that the effused blood hardly coagulates, owing to being diluted by the constant flow of peritoneal lymph, there are no natural hæmostatic processes in operation at all. Fainting is the only means nature can employ to check the vital stream for a little. One fainting fit follows another, but with revival of consciousness comes revival of the flow. The result is that we might almost as well delay about starting artificial respiration in an apparently drowned man, as delay operation in a woman with a constant soaking of blood into her abdominal cavity. Then, when time has been lost in trying to rally her with stimulants, which only remove fainting, dilate the small arteries, increase the force and frequency of the heart's action, and so promote the bleeding, everybody can see the woman is doomed, so that friends naturally refuse to allow the operation, and surgeons

are just as well pleased to be relieved of an operation which would only bring them discredit, seeing they have allowed the woman to become practically moribund. The death in the majority of cases is purely from hemorrhage, as the abdomen full of blood on ante- or post-mortem section shows. That the free space in the peritoneum, and the dilution of the blood with fluid, as it flows, encourage the bleeding, is proved by the fact that, considering the size of the rent in the tube and the comparatively small vessels opened, the hemorrhage is enormous. As much as two gallons of blood have been found in the abdomen, and two or three pints is not an unusual amount. The hemorrhage, indeed, has no relation to the size of the rent in the tube. In fact, to delay the operation is to remove the last chance of recovery. In many cases the patient is in a hopeless condition before operation has been decided on, for, as Lusk says, "*the resources of surgery are rarely successful when practised on the dying.*" In some cases where a little delay for help was absolutely necessary, or in cases where the patient was almost dead before being seen, it might be possible to either prevent the coming on of the moribund state, or remove it enough to give the patient a chance of life after operation. We shall now consider the means to be used to this end.

RESUSCITATION OF PATIENTS AFTER RUPTURE, TO GIVE ABDOMINAL SECTION A CHANCE.

Death does not generally follow rupture for some hours. There is, therefore, always time for operation, except under peculiar circumstances, but when seen, extreme prostration may forbid operation. Should delay be absolutely necessary after the arrival of the surgeon he should make pressure on the abdominal aorta to prevent further hemorrhage, as suggested by Playfair. If collapse be too extreme to give hope of operation, we should try to resuscitate the patient sufficiently to make the operation hopeful. Transfusion naturally suggests itself, but intravenous saline injections are safer, much easier of access and management, and just as efficient. In a parallel case, the algid stage of cholera, Dr. Francisco Aguirre, of Santiago (*Revista Argentina de Ciencias Médicas*, Feb., 1888), produced quite a "resurrection" from a state of profound collapse by intravenous injection of artificial serum composed of 20 grammes of sulphate of soda and 10 grammes of sodium chloride to 2 litres of water. 600 grammes were injected

at intervals into the left saphena vein. Dr. Aguirre refers to other recorded cases of intravenous injection by Lorain, Dujardin-Beaumetz, Rouvier, and Hayem, and urges that the results justify a continuance of this plan of treatment.

Still better, the experiment has actually been tried in collapse from extra-uterine rupture of a gestation sac, and the operation might have been done under favorable circumstances, but the patient was supposed to be cured, which, of course, she was not, so the hemorrhage recurred and carried her off.

In the *Vratch*, No. 33, 1885, p. 543, Drs. J. F. Zemacki and Kotelnikoff, of the Obukhovsky Hospital for Women, in St. Petersburg, give the case of a soldier's wife, aged 31, who had been brought pulseless, insensible, with all the signs of acute anæmia. The patient had been reduced to this state by profuse uterine hemorrhage of three days' duration. The uterus was found slightly enlarged, the os being closed, but discharging some sanguinolent fluid. The subcutaneous injection of camphor and the internal administration of valerian, Hoffman's solution, and wine, brought but slight improvement. In view of the danger immediately threatening the patient's life, the authors resorted to intravenous injection of a litre of a solution of common salt (6 grammes), liquor potassæ (1 minim) and pure carbonate of potash (3 grammes) in water (1000 grammes), heated up to 40° C., and poured through a funnel from the height of a mètre. The transfusion lasted about fifteen minutes. A very marked improvement immediately ensued, the cardiac action becoming stronger, the pulse fuller and less frequent, the consciousness returning, etc. The patient being now able to give account of herself, the authors learned that she had been previously healthy up to three days before, when painless flooding suddenly appeared; the last menstruation had occurred three months ago; nevertheless, the patient did *not* regard herself as pregnant. Having taken all together (including the negative results of a repeated careful examination), the authors arrived at the conclusion that they had probably to deal with a case of *intra-uterine* growth. However, during the subsequent days, there were developed all the signs of acute peritonitis with intra-peritoneal *accumulation of fluid*, and on the fifth day after her admission the patient died in collapse. The post-mortem examination revealed rupture of the left Fallopian tube, which was greatly distended, and contained a

fœtal sac with a large blood coagulum and rudimentary placenta, but no embryo. The peritoneal cavity was filled with an enormous amount of sanguinolent fluid—blood diluted by peritoneal lymph. The authors seemed to think the rupture of the tube took place *after* the patient's life had been saved by the injection of saline fluid.

This is a most instructive case. The authors, though evidently skillful in general medicine, as evinced by the very rational treatment of the collapse, yet seemed to be totally ignorant of the subject of extra-uterine pregnancy. When they found collapse from acute anæmia, they never suspected what the *fluid* was which had suddenly filled the peritoneum, and even after death they did not interpret the case aright. The woman evidently was brought to hospital after tubal rupture, was restored to life, but the bleeding recurred from time to time, till her abdomen was full, when she suddenly died. However, the case shows the possibility of completely resuscitating a patient collapsed through ruptured tubal pregnancy, thus allowing the operation to be done with almost no danger whatever. In the case described the new supply of circulating fluid replaced the old so well that the operation could have been done any time for *five days after rupture.*

As the faintings of acute anæmia are extremely dangerous, it is well to know of some means of restoration. For this purpose hypodermic injections of one or two drachms of ether, or of brandy, have been employed, and are very useful indeed, as a temporary resuscitator, though when the collapse was very great from hemorrhage I do not believe they would revive the patient sufficiently for the operation, as we see the intravenous injection of salines does. However, recently a more powerful remedy for resuscitation of those apparently dead has been hit upon, which might do us good service on an emergency in abdominal hemorrhage. Dr. M. H. Lackersteen (*Arch. für Gynäk.*, July, 1888) publishes three cases in which patients apparently dead were resuscitated by means of hypodermic injections of nitro-glycerin. The first case was that of a young lady who became suddenly collapsed during the passage of a gall stone. Neither respiration nor pulse were perceptible. The hypodermic injection of ten minims of a one per cent. solution of the drug was promptly followed by signs of returning life, and she ultimately made a good recovery. Case second was one of *drowning,* and case third was that of a *still-born child.* In each case atropine was injected as soon as the effects of the nitro-glycerine were apparent.

THE OPERATION FOR RUPTURED TUBAL GESTATION.

The objects are the arrest of hemorrhage, removal of the blood already in the peritoneal cavity, ovum and ruptured tube. The incision should be made in the middle line, and midway between the pubes and umbilicus, and should be about one and a half or two inches in length. The linea alba is exposed at the second or third stroke of the knife. Pressure forceps are applied to any bleeding points and left attached. A small opening is made in the fascia, the fibrous septum between the recti found, then the fascia is slit up, the point of the knife directed *forward*. The muscles are pushed aside with the handle and the sub-peritoneal part found. The fat is lifted up with two pairs of catch forceps and carefully opened. While the forceps pull the tissues up the peritoneum can easily be made out; a small opening is made in it while thus elevated, the finger is inserted, and the remainder of the incision made on the finger by scissors. By this method the bowels run no risk, as each tissue is lifted up before division; then the first little hole made in the peritoneum admits air, and the bowels fall quite away from the parietes. The index and middle fingers are now inserted. If the omentum cover the intestines it must be worked up by the fingers. When the way is clear the fingers are passed right down to the fundus uteri. At the first incision through the peritoneum blood will probably escape and the pelvis be found full of blood and clot. The fingers have to find the fundus uteri among the clots. Then with a finger on each side of the broad ligament, the Fallopian tube is traced first on one side, then on the other. The gestation sac will be made out as a soft enlargement, of varying size, according to stage of gestation, and the consistence will vary according to whether the ovum has escaped or not. The sac is brought to the surface and the tear searched for. The best site for a pedicle is selected, ligatures put on and tied in the Staffordshire knot, or, if the pedicle is very broad, two Staffordshire knots or a chain ligature may be required. The whole sac is then cut away. In this way we need not trouble about stopping bleeding at the torn spot. The ovum is seldom found complete in situ, the placenta is often found in the sac, but both fœtus and placenta are often free among the clots. The peritoneal cavity is then cleared as far as possible by the fingers or hand; then irrigation with hot water is employed, and, lastly, the cavity dried by the sponge. To clean out the peritoneum it may be necessary to enlarge the incision;

a little flat sponge is laid upon the bowels under the incision to protect them during the stitching, and the sutures, four or five, are placed in position. The sponge is then removed and the sutures tightened and tied. The operation is a most easy and simple one, and need not occupy more than ten minutes. As this is not a treatise on abdominal surgery, I think it would be a mistake to load it with purely surgical details as to operations, use or non-use of antiseptics, precautions as to cleanliness, details of after-treatment, etc. All these are discussed in every work on operative surgery. Suffice it to say, that the operation should at least be *aseptic*, if not *antiseptic*, in all its details; the hands, instruments, sponges, dressings—everything touching the wound or surrounding the patient—should be scrupulously clean. This is confessed to be the secret of the antiseptic system, which has done glorious service in teaching us the absolute necessity of these things if we are to have any success. Indeed, let leading surgeons say what they will, it is the antiseptic system they have to thank for all the modern advances in surgery. We have now found out what parts of it are essential and what needless, or perhaps injurious, that is all. But, perhaps, as I have used a term, "Staffordshire knot," which may not be familiar in all parts of the world, I had better explain that simple contrivance. The Fallopian tube, when the diseased one is found, is pulled out tight, and the broad ligament spread out ready for the ligature. The ligature is passed double by means of a very blunt needle with the eye at the point, which is then withdrawn. The ligature is made to include the tube and as much of the broad ligament tissue as may seem necessary to include all vessels supplying the bleeding part. When the needle is withdrawn we have a loop of the ligature sticking through the broad ligament, the two free ends are lifted and brought round the Fallopian tube, and *one* of the ends is put through the loop and both ends then pulled tight with one hand, while the fingers of the other steady and make counter-pressure on the Fallopian tube. At this stage we have the loop with one end through it at the one side of the Fallopian tube, the two ends coming round the free border of the tube, one tucked through the loop and one not; then the two ends are taken up and treated just as if it were a simple single thread we were about to tie round the tube, that is, a reef-knot is cast on the ends, perhaps with the aid of an assistant pulling one end, so that the surgeon may keep the knot close down on the tube. The purpose

of the first part of the knot is to make sure that it is tight enough to arrest the circulation through the vessels by a mere pull, as the surgeon is working out of sight among tissues slippery with blood. When the one end is put through the loop and both ends pulled, the tube is tightly encircled, and the tightened loop does not relax when the ends are relaxed, although a further knot is added to make sure the loop does not relax in time.

In many cases the diagnosis has to await the section; the rule must be to operate when signs of alarming abdominal hemorrhage occur. We should never allow a patient to die because the diagnosis is not completed. This has always been the objection urged against laparotomy hitherto, viz., the uncertainty of diagnosis. But, surely in these days, when men insist that it is quite easy to diagnose extra-uterine pregnancy before rupture, the old excuse will fail. The diagnosis is of no importance as to the treatment; we operate to arrest intra-peritoneal hemorrhage, the alternative being death, and knowing that *intra*-peritoneal hemorrhage is usually due to ruptured tube.

TREATMENT OF EXTRA-UTERINE PREGNANCY BETWEEN PRIMARY OR TUBAL RUPTURE AND THE FULL PERIOD OF GESTATION.

"If the woman does not perish from rupture of the cyst during the first four or four and a-half months of pregnancy, it is not likely that an opportunity will offer to inspect the body until at, or near, or even some time after, the close of pregnancy." Tait's theory, as I have said, is that, almost without exception, fœtuses which survive tubal rupture have been discharged between the folds of the broad ligament. These folds are easily opened out by the growth of the fœtus, so that often the fœtal sac comes to encroach on the general cellular tissue of the pelvis, strips up the peritoneum from the back and sides of the pelvis, the back of the uterus, and even from behind forward, until the whole lower part of the anterior abdominal wall is stripped except a piece between the cornu of the uterus and extending down to the base of the bladder. Tait does not deny the possibility of abdominal pregnancy. He only says it is rare, and explains how it comes about. Jessop's case is a proof that abdominal pregnancy can exist. Nonat's case, 1855, and that of Duverney, 1712, show that the account Tait gives of their origin also occurs. The post-mortem record in Nonat's case is most important in another way: it

illustrates one of the dangers of extra-uterine pregnancy after primary rupture (supposing it to be in the broad ligament). In this case 25 ounces of black fluid blood and clot were found—*intra*-peritoneal hæmatocele, and as usual an almost sure cause of death. When the intra-peritoneal blood was removed, an ovoid tumor was observed, covered by the peritoneum of the broad ligament. This tumor was formed of blood also, with a fœtus added; it was what Tait holds to be the usual form of extra-uterine pregnancy where the case goes beyond tubal or primary rupture. It was, in fact, an extra-peritoneal hæmatocele plus a gestation sac and fœtus. But the order of events was plain. The extra-peritoneal hæmatocele and gestation had been in the broad ligament, but the broad ligament had given way and poured blood into the cavity of the peritoneum, and this had caused death, as no operation was done. At the bottom of the left recto-uterine cul-de-sac, the peritoneum forming the posterior layer of the broad ligament presented a perforation with a communication between the recto-vaginal cul-de-sac and the cellular tissue separating the peritoneal layers of the left broad ligament. This case points out the treatment when this "secondary" rupture occurs, as it may occur at any moment from primary rupture till the fœtus is removed by operation or eliminated by nature. The woman is liable to secondary rupture into the peritoneum and to death therefrom any day while she has an extra-uterine —generally called an abdominal—fœtus inside her. But operation would save her when this happens at any time, just as in the case of primary rupture. At least the chances would be immensely in her favor, whereas, without operation, her death is almost certain. The operation in secondary rupture would be different, and rather more difficult than that for primary rupture. Abdominal blood would be cleared out on incision, then the secondary rupture searched for, the gestation sac incised, clots and fœtus removed—fœtus often will be absent, from absorption—any hemorrhage still going on from interior of sac stopped by styptics, as vinegar, actual cautery, or ligature to actual bleeding points. Then when the sac had been cleaned and dried, the edges would be returned to the edges of the incision in the abdominal walls, a drainage tube put in, and the wound closed except at the bottom. If a definite placenta be present in these cases it should not be touched, as great hemorrhage is likely to take place from its site unless the fœtus has been long dead, and there is no

efficient means of arresting the hemorrhage. The woman will not be out of danger, of course, till the placenta has come away in the discharge along with the fœtal membranes, and the great point of the after treatment is to prevent septic absorption. This is done on general surgical principles, on which I need not enter, as they are in no way special to my subject.

In the *Brit. Med. Jour.*, December 15th, 1888, p. 1339, will be found a discussion on a case of extra-uterine gestation before the London Obstetrical Society. As this may be considered the very latest expression of opinion I notice it. Dr. Griffith asked if the case were proved to be extra-peritoneal according to Tait's pathology, as there was a specimen in St. Bartholomew's museum which almost disproved the theory. Now, how one specimen could disprove the theory that the majority were extra-peritoneal is hard to see, and the discussion as a whole seemed to considerably corroborate Tait's theory. Griffith further asked if benefit seemed to arise from the median incision (the case was one operated on eight months after fœtal death) and the opening of the peritoneal cavity, as it had been *shown by Mr. Thornton and others* that it was *usually better* to *cut down directly on the sac* and *avoid opening the peritoneum.* Here, it seems to me, Dr. Griffith either stultifies himself or shows he knows nothing whatever of the subject. How, in the name of common sense, could Mr. Thornton show that it was *possible*, let alone *better*, to avoid opening the peritoneum if the fœtus were *within it*, as he alleges? How could the fœtus be removed without opening the cavity in which Dr. Griffith maintains it is?

Again, Dr. Champneys said that the statement that all cases of extra-uterine gestation which went quite, or nearly, to term were intra-ligamentous was unfounded. In a case by himself ("Obstet. Soc. Trans.," 1887) the fœtus was simply kicking free among the intestines. Very well; but what then becomes of the theory about peritonitis and encystment? It is probable that the fœtus was quite free through all its career? The difference between Dr. Champneys and Mr. Tait is that Champneys offers a very rare case as a refutation of Tait's theory, but does not attempt its explanation, while Tait points out that such is a rare exception, and explains it, as he has done Jessop's very similar case. It seems to me that the least objectionable explanation of Jessop's and of Champneys' cases is that offered by Tait, viz., that in such cases secondary rupture sets the child free in the abdomen; that its

original situation from the time of tubal rupture had been external to the peritoneum, but that the distended broad ligament had given way and the fœtus slipped into the peritoneum. In such cases hemorrhage, for some unknown reason, must have been slight, or the woman would have died of abdominal collapse, as after primary rupture. Dr. Cullingworth, in reply, gave reasons for believing the pregnancy was *originally* tubal, and had *become a broad ligament gestation* through rupture of the tube. Operation *without opening the peritoneum* would have been impossible in this case, owing to the relations of the parts. I cannot give more space to this most recent discussion on my subject. My conclusions, from a study of it, are that there must be a great amount of truth in Tait's theory, when this sort of thing is all that can be urged against it by his rivals, the London obstetricians, some of whom hardly seem qualified to discuss the question at all. A man who maintains that abdominal pregnancy, in the old sense, is the rule, and then asks if the advantages of *removing the gestation without opening the peritoneum* are as great as alleged, is certainly an opponent who will not greatly injure (or benefit) any cause.

When a woman has recovered the shock of the primary rupture, which often, in these cases, is too slight to be noticed, the case then becomes practically an extra-peritoneal hæmatocele, and unless the signs of pregnancy be very numerous and decided, it could not be diagnosed from simple hæmatocele until the fœtal heart sounds become audible. The treatment should be symptomatic. If possible, the case should be let alone, or such palliative and sedative treatment adopted as pelvic pain, obstructed defæcation, or obstructed micturition may call for. The woman is perfectly safe during this period, save for possible accidents which, however, may occur at any time. The dangers of broad ligament hæmatocele with fœtus or without it are—secondary rupture of broad ligament into peritoneum, usually causing fatal hemorrhage unless abdominal section be done at once; when inflammation and suppuration occur in sac; and when fœtus, having died, when the case is due to gestation, nature tries to eliminate the fœtus by fistulous openings in various directions. Suppuration in hæmatocele from gestation is not very common, as the gelatinous fœtus is often completely absorbed. In forty cases where Tait removed a placenta he only found twelve fœtuses ("Lectures," p. 38). This is too large a propor-

tion to be accounted for on the theory that he had overlooked the fœtus among the clot and débris.

The treatment between the primary rupture of the Fallopian tube and the viable period should therefore be expectant. Laparotomy should be done in case any symptoms arise putting the mother's life in danger. Laparotomy is always safer than any other kind of interference. The objections to tapping the gestation sac through the vagina or abdominal wall, to electricity whether with puncture or without, are a hundredfold increased as the fœtus grows larger. If there be any excuse for killing the fœtus in the very early stage, there is none now. Nature, in the majority of cases kills the fœtus, but safety is not often procured to the woman. Should a woman carry a dead fœtus for fifty years she is always in great danger, always at the mercy of accidents. She may take ill with symptoms of abdominal collapse at any moment, and the alternative then is laparotomy or death. No puncture, no electric current, will restrain bleeding from an abdominal artery; nothing will do but an operation. So such cases should be nursed up to full time, if possible, we being prepared, of course, to interfere at once if necessary. Pelvic abscess, if it occurs, should be opened from *above* the brim, as this gives best results in modern times, and, on the first sign of fœtal remains being eliminated the sac should be opened *above* and cleared out. I cannot enter into the advantages of this modern practice over the older let alone policy, within the limits of this essay. I shall merely say that the arguments will be found in Tait's "Lectures on Ectopic Gestation," at page 36. It will be found that Tait operates to prevent a pelvic abscess causing fistulæ, here and there, as this involves immense suffering, years of invalidism, and death in the end, in a large number of cases. He has opened the pelvis above in about thirty cases, with *recovery in all.* He says he can't say surely how many of these suppurating hæmatoceles were due to tubal pregnancies, but he suspects more than half were, as this is perhaps the greatest cause of hæmatocele. Here Tait is by no means alone in his opinion. Gallard (*Gazette Hebdomadaire*, 1858, p. 461) thought that most hæmatoceles were extra-uterine gestations. Lesonef, who was the great authority on extra-uterine gestation before Parry, is ("Remarques sur Trois Cas de Grossesse Extra-uterine," p. 18) of the same opinion. Leopold (*Arch. für Gynäk.*, Band xviii, S. 74) says: "How

often may it happen that rupture of the gestation sac . . . . in the second month may run its course with the clinical phenomena of a retro-uterine hæmatocele, and end favorably with complete absorption of the effused blood? I do not for a moment doubt that such hæmatoceles occur much oftener than we usually think, and Schroeder, (*Lehrbuch der Geburtshülfe*, 6th Auf., S. 408) believed that he had seen cases of tubal pregnancy run a favorable course in this way so often that he believed termination in cure to be the rule." Veit (*Zeitsch. für Geb. und Gyn.*, Band xi, S. 384) goes so far as to declare, "that extra-uterine pregnancy is the *most frequent of all tube diseases* for which patients come for treatment; that all cases of hæmatocele occurring in women who think themselves pregnant, or have suffered from irregularities in menstruation before the illness, *should be taken to have arisen in* tubal pregnancy; and that cases of *apparent parametritis after* abortion should be suspected of being *hemorrhage from extra-uterine pregnancy*." Herman has seen three cases of large hæmatoceles, preceded by a history pointing to extra-uterine gestation, and accompanied by the passage of a decidua, in each of which the patient recovered. Indeed, Herman (*Lancet*, May 26th, 1888, p. 1022) has seen many cases in which, with the signs and symptoms of hæmatocele, there was a history more or less consonant with the supposition that extra-uterine gestation had been present.

The diagnosis in these cases, though far from certain, yet rested upon grounds quite as strong as those supporting the diagnosis in the great majority of the cases reported as having been cured by electricity. On this supposition, that so many cases which escape the fatal primary rupture recover by early death and absorption of the fœtus, we build one of the heaviest arguments against electricity, tapping the cyst, etc., and also give an explanation of the supposed successes by such methods. When the fœtus undergoes any great development, however, danger properly only begins when the fœtus dies. From some obscure reason, some fœtuses remain for a long time—perhaps many years—quite fresh and undecomposed. In such the sac does not much incline to suppuration without some traumatic cause added, as a blow. In other cases, probably where the placenta adheres to bowel and septic gases pass into the fœtal circulation, the fœtus soon decomposes, inflammation commences in the sac, septic absorption ruins the

mother's health and threatens her life, and abdominal section becomes the only rational treatment.

Dr. R. P. Harris has inquired into the subsequent histories of the cases that have been *cured* by electricity. Some of these, including one of Dr. Allen's, had suffered from repeated attacks, supposed to be peritonitis, and continuous ill-health for years. A dead fœtus cannot be an innocuous mass unless in the very early stage, and not always then (see *Brit. Med. Jour.*, Dec. 4th, 1886, p. 1094). Routh, although an advocate of electricity prior to tubal rupture, says that from that period up to the viable age, we should open the abdomen to prevent possible rupture (secondary) (*Brit. Med. Jour.*, as above) This seems to be uncalled for except circumstances demand it.

Dr. Gill Wylie, New York (*Brit. Med. Jour.*, Dec. 4th, 1886, p. 1093), reports that he did abdominal section on a woman who had been previously subjected to electrical treatment and *reported cured;* and Dr. Gordon, Montreal, admits (*ibid.*), although the fœtus may be killed by electricity, it may *not prevent the necessity for subsequent abdominal section.* Indeed, Dr. Petch, after reporting his famous case where he caused the death and absorption of a six months' child, pleads that even if it be necessary afterward to remove the dead child by abdominal section, the condition will have been in no way made worse for operation by the previous use of electricity. He also admits that in his famous case the dead fœtus would have been a great danger for a year after, in case the woman had conceived again. But Petch used galvano-puncture, and I can't admit the truth of his argument, that in any case the galvanism can do no harm, as I fancy I have proved that any puncture is dangerous—if not quite as dangerous as abdominal section. And if he did not kill the patient at the time by hemorrhage or septicæmia, she would be more liable afterward to decomposition of the fœtus on account of the puncture. The danger of septic mischief would continue until the whole results of gestation were completely absorbed (*Edis, ibid.*). All authorities seem agreed in discountenancing electricity or puncture after the sixth month.

Conclusions.—It seems, then, to sum up the treatment of extra-uterine pregnancy between the primary rupture and the viable age, that there are only two alternatives worthy of consideration, viz., expectancy and laparotomy.

The life of the child is an element which should enter into the consideration, and in the absence of symptoms endangering the mother's life, and as operation *before* the full period is no more dangerous than before the fœtal sounds are heard, we should give the child a chance for its life. Why should we destroy one life without any corresponding advantage to the other? We should, then, leave the case severely alone, enjoining on the mother precautions as to the avoidance of exertion and all manner of accidents likely to provoke secondary rupture, or death of the child. When operation is rendered necessary by rupture of the sac into the peritoneum, the fœtus is generally macerated, so that it is probable that fœtal death makes all the circumstances demanding operation more likely, as in normal pregnancy fœtal death is the great cause of abortion.

TREATMENT OF EXTRA-UTERINE PREGNANCY AT THE FULL PERIOD.

The mother should be carefully guarded and watched till false labor sets in, and the onset of labor seized as the most favorable time for both mother and child.

The statistics of this operation, until recent times, have been very discouraging. The operation during the life of the child, was called the "primary" operation, in contrast to that after its death, which was called "secondary." But when all vital action is over and the circulation through the placenta ceased and succeeded by thrombosis of the maternal sinuses, opening the sac is hardly more dangerous than opening an abscess. Parry reported twenty cases of "primary" operation, with eight children saved, and six mothers, but, according to Lusk ("Midwifery," p. 338), the record is worth nothing, as to the mothers at least. Litzmann ("Zur Feststellung der Indicationen für die Gastrotomie bei Schwangerschaft ausserhalb der Gebärmutter") gives nine operations, with only one recovery—the now famous case of Jessop. Werth ("Trans. of the Gynæcol. Section of the Eighth International Congress at Copenhagen") collected seven cases, with one recovery—that of Martin, of Berlin. Litzmann reports that only four children survived the third day, out of ten operations. Koeberlé reports nine cases, seven children and four mothers being saved. Meadows related a good case to the Obstetrical Society (November, 1883.)

But this alarming mortality must not be taken as that which is likely to occur in the future. Many reasons conspired to give results such as the above. First, no tables of operations previous to 1878 are of much value, as the final abandoning of the clamp in ovariotomy revolutionized abdominal surgery. In the operations before this date, the peritoneum was fouled in the operation and not cleaned out. The only hope of the surgeon was in "adhesions," which would shut off the gestation sac from the remainder of the cavity of the peritoneum. It is now known that the danger lies not in opening the peritoneum, but in allowing decomposable matter to escape into it, or, if it gets in, neglecting to wash it out again. The peritoneal cavity being a gigantic lymph space, putrescible matter might as well be injected into a vein as left there. Then the older statistics are of little real value from another reason. Most of the patients were operated on "in extremis," and hardly two were by one man, or agreed in anything. Hardly two had anything in common for comparison, yet they are all thrown together as if nearly similar.

It may be said that in these old days the "secondary" operation, after fœtal death, gave excellent results, although the same want of skill in abdominal surgery, the same want of asepticism, prevailed. Yes; but in these chronic cases the adhesions did cut off the peritoneal cavity, as a rule. A great many of them were done after suppuration of the sac, and the fact that the woman still lived—most of them died before this stage—showed that the peritoneal cavity was shut off, or the pus would have poisoned the patient. So, the woman was hardly susceptible to the want of cleanliness in the operation. Besides, the "puerperal condition," to which Parry and others attributed the great fatality of operations at term, certainly did make the woman much more susceptible to bad surgery and ignorant operators. But, without taking bad surgery into account, septic influences are fatal to the woman in the "puerperal state." Tait says he never succeeded in getting a woman through a Cæsarean section, and he thought it was the puerperal influence. But since he began to amputate the uterus at the same time, and thus get rid of metritis and its septic consequences, all his patients recovered easily, just as they recover in ovariotomies; so he now concludes the puerperal influence is all nonsense. ("Lectures," p. 78.) Taken as a rule of practice, the comparison of the "primary" and "secondary" operations was quite delusive.

This method compared together the fatality attending operation before the death of the child with operation at a time when large numbers had already died. It compared a woman's chances when operated on during the life of her child with the chances of women to survive a final stage of deliverance, but took no account of the numbers who had already perished. Parry himself says: "During the discharge of the decomposed child the mother is subjected to all the dangers which result from the absorption of purulent and putrid matter." He has tabulated 330 cases, and of these 105 died without any operation. This gives a mortality of 31.8 to start with, and, if we are to truly estimate a woman's chances of life, this should be added to the results of the secondary operation, as the primary operation gives a final cure when recovery follows. That this argument is not mere advocacy of operation may be shown by the fact that, unless a long time is allowed to elapse after fœtal death, the operation is still nearly as dangerous as the primary one. So, if we are to give our patients the safe operation, we must wait till 31.8 per cent. of them would be dead. All of Litzmann's patients who underwent operation during the first month after fœtal death (seven) died, but, in cases operated on at from five weeks to twelve months after fœtal death, only seven died out of twenty-six. Now, in modern times, Tait has operated on seven cases, saving *five* mothers and three children ("Lectures," p. 35.) This is not much greater than the mortality in Litzmann's cases of secondary operation; Tait's mortality for the dreaded primary operation being 28.5 per cent., Litzmann's, for operations from five weeks up to a year, 26.9 per cent., while the primary cases and those for a month after fœtal death *all died.* This shows the progress we have made in abdominal surgery in a few years. Of course, the figures on both sides are much too small to base conclusions on, but they seem to indicate great advances in surgery. The deaths of all the cases operated on in Litzmann's "primary" list were due partially to the desperate state of the patients necessitating early operation, and partly to bad surgery provoking profuse hemorrhages from removal of the placenta.

There are even yet no means of knowing when the thrombosis of the maternal sinuses renders hemorrhage from the placental site unlikely. Schroeder removed the placenta three weeks after fœtal death and had no loss, while Depaul, in an operation at four months after

fœtal death, had *fatal* hemorrhage. Modern surgeons, however, leave the placenta alone, as a rule, Martin, of Berlin, excepted. Sometimes, indeed, it is found in the line of incision, when more or less separation is unavoidable. As a rule, the placental attachments are in the pelvis. The placenta may adhere to the bladder, uterus, omentum, intestines, etc., but not often to the anterior abdominal wall; of course, the operator might encounter it on opening the gestation sac. Lusk says that the placenta has been found in the line of the abdominal incision in the proportion of one case in six ("Midwifery," p. 339), but he must refer to borders of it only, as it is far more spread out in extra-uterine pregnancy than in normal cases. But the figures of Litzmann's secondary operations which I have above compared with Tait's are quite too favorable to be used for that purpose, as they evidently include operations which were merely enlargements of fistulæ. If we take the cases he has collected of secondary laparotomy, as distinguished from that performed during the life of the fœtus on the one hand, and incisions for the purpose of extracting fœtal bones from fistulæ on the other, even cases collected in recent years fall far below the success of Lawson Tait in the primary operation. Litzmann collected thirty-three cases of secondary laparotomy (twenty-four of them between 1870 and 1880); of these nineteen recovered and thirteen died, giving a mortality of 42.4 per cent. against Tait's mortality of two cases out of seven = 28.5 per cent. Koeberlé saved four mothers and seven children out of nine operations, giving a maternal mortality of 55.5 per cent. As far as it is wise to draw conclusions from such very small numbers, the primary operation in the hands of Tait seems to be in the position (or better) in which the secondary operation was in the last decade. Of course in good hands the secondary operation has also greatly advanced. Gaillard Thomas saved all four lives in as many operations, and Tait only lost one out of seven.

Lusk, in a paper entitled "A Plea for Early Operation in Extra-uterine Pregnancy," tells us he did not collect his figures with a view to supporting any foregone conclusions. He gives us 103 cases, occurring between 1875 and 1886, excluding cases of rupture in the early stages. Of twenty-nine cases of "abdominal" pregnancy terminating in fistulous openings, nine died. These are often looked on and reported as cases of *spontaneous cure;* and if to the actual death rate the highly probably permanent injury to health and usefulness is added, it will be

seen that natural elimination of the fœtus is not a very hopeful lookout. Eight cases died before fistula had formed, at from eight months to a year and a half. Of fifty-two cases of secondary laparotomy at different periods after fœtal death, thirty-seven recovered and fifteen died. In only three of the operation cases could death be attributed to the laparotomy; these were all cases of profuse hemorrhrge. In all the other cases, fatal after operation, the patients were almost moribund. Although operation is not usually successful upon the dying, still some cases recovered which seemed past all hope. It is often difficult to say under which class an operation should be placed, as Litzmann says that furious hemorrhage has occurred from the placental site four months after fœtal death (*Brit. Med. Jour.* Dec. 15th, 1888, p. 1340). Such a case would have all the dangers of a primary laparotomy.

Medical opinion at the present day is mostly in favor of not awaiting fœtal death, but of giving mother and child both the best chance for their lives. Tait's latest utterance on the subject (*Brit. Med. Jour.*, March 10th, 1888, p. 535) is "that *all* children and *most* of the mothers might be saved by proper surgical intervention at the opportune moment." Dr. Playfair, at a debate before the Obstetrical Society, London, on Dec. 5th, 1888 (*Brit. Med. Jour.*, Dec. 15th, p. 1340), said that Dr. Harris's tables of cases of primary operation showed a most gratifying success. Primary laparotomy has been advocated by Velpeau, Kiwisch, Koeberlé, Schroeder, Barnes, Levret, Gardieu, Meadows, and, indeed, most recent authors. Jonathan Hutchinson and others prefer the secondary operation. If we delay operation the cyst may rupture during the efforts at labor, and death occur at once, or other risks, which cannot be predicted, are always liable to occur. The woman may sink from peritonitis or exhaustion consequent on rupture of a suppurating sac, which in nearly all cases sooner or later occurs, so that "the patient's life may be said to be at the mercy of accidents, of which we have no sufficient warning." By delay we deliberately sacrifice all hope of saving the child, while it is quite doubtful whether we increase the mother's chances in the long run. Indeed, in the opinions of all modern writers of the least intelligence, the question is between primary and secondary laparotomy; nobody recommends abandonment of the case to "nature," nor the allowing of a woman to go about with even a quiescent gestation in her abdomen for the remainder of life. The best that Parry can say

for his do-nothing policy is that when the case is left until nature has shown through which channel elimination is to be effected, the mortality is 17.35 per cent. less than in the cases in which the primary operation was postponed. He should, in justice, add on the large mortality which occurs during the process of elimination, which is, according to Lusk, nine deaths in twenty-nine cases allowed to proceed to formation of fistula. According to Parry himself, out of 330 cases, 105 died trying to eliminate fœtal remains. If we consider the risks of septicæmia, etc., which women run from the death of the fœtus until it is eliminated, which must happen in most cases, unless death anticipate it, I think we shall conclude that primary operation is safer than total neglect, not to speak of the immense saving in suffering and loss of usefulness. But to also omit secondary laparotomy, in these days, would be almost criminal. Parry himself saw this, as he says: "After the death of the fœtus, and the restoration of the normal condition of the system, the retention of an extra-uterine fœtus is not incompatible with a long and useful life, but a woman is *never free from danger* while she is carrying an encysted child. Violent exercise, injuries, blows, strainings, and similar mechanical irritations, may be the exciting cause of inflammation of the sac at any time. Hence, violent pain, with fever and evidences of inflammation following these, always demand a cautious prognosis. Depressing diseases, as any of the continued fevers, or local affections which introduce a profoundly typhoid condition, endanger the woman by impairing the nutrition of the cyst, and leading to destructive inflammation."

The great defects of Parry's tables are that the survivals he compares with the cases operated on, are the mere remnants of the hosts which have died before the period of comparison, and that the mortality in his day was wantonly, as it were, run up by the most stupid delay until recovery was well nigh hopeless, and a great deal of most unscientific bungling before the case was resigned to operation, as a last resort; for instance, one case was operated on after over a week of labor, and another after most persevering efforts for a *whole day to deliver her with forceps!!* The operation should not be left till it assumes the character of a last resort; it should be looked on as a mode of treatment. Harris gives a case in the "International Cyclop. Surg.," vol. vi, p. 784, which illustrates this:—

"In October, 1885, three prominent Philadelphia obstetricians met

daily in consultation for sixteen days over the case of a lady who was suffering the pains of false labor. . . . . As they could not promise the husband that an operation would probably save the life of his wife, they waited for the time to come when they could do this; but, while doing so, and when the lady appeared to be getting better, she was suddenly seized with agonizing pains, followed at once by a state of collapse, and died in thirty minutes."

THE "PRIMARY" OPERATION.

On account of the re-arrangement of the peritoneum—to avoid opening it if possible—the incision should be made well to one side of the middle line. The long tube-like process of peritoneum extending from each side of the fundus uteri to the base of the bladder, would be opened by a medium incision. No doubt this has occurred many times when the gestation cyst presented close behind the abdominal muscles, a little to one side, and so gave the operators the idea that most of the extra-uterine fœtuses were *in the cavity of the abdomen.* The history and the physical signs will show which side the gestation is on. The incision should be two or three inches from the middle line of the body. When the sac is opened the fœtus is to be carefully lifted out by the feet, using great care not to lacerate the sac or abdominal wall. If the child is living some one disengaged should take immediate charge of it. Another important feature in the latest form of operation (Tait's "Lectures on Ectopic Pregnancy," p. 95) is, that the umbilical cord should be cut off quite close to the placenta, the placenta squeezed as empty of blood as possible, the sac cleansed of all blood, loose membranes, etc., and then washed with warm water, the sutures carefully placed, the sac again washed out with clean water by means of a siphon trocar, and the stitches drawn tight, with a small trocar still in the wound. The sac should then be emptied of all the water possible, the trocar taken out with precautions against admission of air, and the *wound totally closed.* This is quite a new operation, and promises to be a great advance on any yet tried. Tait says he thought of it in consequence of the striking success of the same operation in congenital cysts. The results, in case of cysts, are much better than where drainage is employed. Tait has tried all ways of dealing with the placenta, and thinks this best. He has removed it by vaginal section disastrously. He has twice removed it in cases where both

the mothers and the children still survive. The hemorrhage in these cases was easily arrested by perchloride of iron; but in these cases he found it possible to tie a big pedicle of the remains of the tube and broad ligament, which contained most of the blood vessels, to the placenta. He thinks it is often possible to tie a pedicle with the main vessels in it, and it should always be done, and under such circumstances the placenta should always be *removed.* But in many cases the placenta is spread out on various tissues, and is so intimately adherent that it could only have been removed with great difficulty and hemorrhage. Tait thinks under such circumstances, although it could be removed, yet it is more prudent to let it alone. In three other cases he kept the placenta in situ, bringing the umbilical cord out at the lowest part of the wound and closing up all but this part. The whole three mothers recovered. In two of them it was unnecessary to open the peritoneum. In one it was opened, and in this case it was carefully closed again, except a little hole for the umbilical cord and the drainage tube. These patients had all to pass through an offensive course of suppuration, which lasted several months and came near killing each of them. Tait believes by his new operation he can get rid of all this suppuration in most cases. The histories of cases where extra-uterine gestation has gone on for long periods show that all the tissues of the fœtus except the bones can be absorbed, and even the bones to some extent. The placenta is the first gestation tissue to disappear, although it continues to grow for a while after fœtal death in many cases. But should the placenta grow and come to require subsequent removal, Tait says it would be an *immense gain* to the woman's chance of life to leave it over for a second operation when her health was restored. So he says "deal with the fœtus only at the first operation." In fact, by this method Tait almost converts the "primary" into a "secondary" laparotomy; he gets the advantages of each, and avoids the drawbacks of each. One great drawback of the ordinary primary operation, when the wound is not hermetically sealed, is that decomposition is wantonly induced in the sac as a means of getting rid of the placenta, which, in the majority of instances, it is too dangerous to remove. The necrosis of the placenta is evidently a fearful risk to a woman in the puerperal condition. I think it will be admitted that a woman during the puerperal period is far more sensitive to all septic influences than women are at other periods; hence surgery which

permits of sepsis is apt to prove fatal to the puerperal woman. The reasons, perhaps, for this sensitiveness are not far to seek. The modern discovery of leucomaines and ptomaines in the healthiest state gives us the clue. As soon as involution of the uterus commences, waste matters are thrown into the blood in greatly increased quantity. These products of retrograde tissue changes have to be *oxidized* into substances which the organs are capable of eliminating. During this activity of the chemical processes of the body, no doubt more leucomaines and ptomaines are formed than is natural. In fact, during the puerperal state the woman is at the verge of poisoning, under the best circumstances; then, if, in addition to these self-bred poisons, others from decomposition are added, the poisoning becomes visible as some acute disease—septicæmia, or some fever with inflammatory complications. It may be objected that this is only hypothesis. This criticism is hardly true, as we now know that poisons are constantly being thrown out of the body by urine, sweat, bowels, and breath. We also know that anything which increases tissue change, as exercise, increases the amount of leucomaines excreted. We also know that if by excessive exercise of any kind we increase the production of poisons beyond the ability of the various organs to eliminate, disease is produced, as a simple or severe fever. We also know, and here there is a perfect analogy with the puerperal state, that men over-marched or over-fatigued, fall easy victims to cholera, or any other infection. The fatigued man is excreting self-produced poisons to the top of his power; if you throw in any more poison he fails to eliminate it, and perhaps it works in some zymotic way on his waste-laden blood, at all events, he takes infection very easily. The puerperal woman is excreting poisons to the top of her power, throw in another, and you very easily cause disease. It is well known how sensitive puerperal women are to the poisons of scarlatina, erysipelas, sewer-gas, and especially septic matter in the genital canal. Is there not more than theory, then, for delaying the decomposition of the placenta in primary laparotomy by closing up the wound? A case by Dr. Champneys ("Obstet. Trans.," 1887) illustrates the principle. The placenta was emptied of blood, but the cord was not cut short, but brought out of the wound, as it was thought the antiseptic system of dressing would prevent decomposition. The operation was done on the 19th October, and on the 19th November "the incision was completely healed, but the hypogastrium markedly

distended, and a swelling, which was supposed to be the placenta, was markedly smaller." It was soon evident, however, that there was decomposition of the placenta, and peritonitis in consequence. The patient was very ill: pulse, 114; temperature, 104° F., and even 106° F. She got worse till the 7th January, when death took place. On examination the placenta was found lying in the sac like a round ball as large as a fœtal head, and of dark maroon color. The placenta was quite detached, save for a few bands and one or two adhesions. The patient could easily have been saved by re-opening the abdomen at the outset of serious symptoms and removing the placenta. Serious symptoms did not begin for five weeks after operation. She was allowed to suffer from septic poisoning for nearly six weeks without the slightest effort to remove the mass of decomposition.

The lesson is to close the wound hermetically to give every opportunity for absorption of the placenta, without decomposition. But should it not be absorbed, and, still more, should symptoms indicating decomposition come on, it should then be removed. This secondary removal of placenta would, probably, be both easy and safe; hemorrhage would be very unlikely, as thrombosis of the maternal sinuses would have occurred. Tait does not pretend that the question is yet settled by experience, but he claims that this is the most rational way of doing laparotomy for extra-uterine pregnancy at the full period or afterward, and all his experience in abdominal surgery of all kinds leads him to expect it will be a success. He says it is certainly not a question which will be settled by the tabulation of a number of cases, mostly dissimilar in the most extreme degree from one another, and incapable of leading to anything but confusion when put in the form of statistical evidence. All must admit that his description of the getting up of statistics of an operation, or of a disease, is only too true, and that, usually, the conclusions drawn, and rules of practice laid down, from an analysis of such jumbles of all sorts of things thrown together, are most misleading, and effectually prevent the advance of medical science. Simple ignorance is safe and hopeful, compared with error. An erroneous rule of practice, or an erroneous opinion based upon a lot of figures arranged so as to lie, remains in our text-books until it is learned by the whole profession. It probably holds the field for years, and when it is at last doubted, no heed is paid, for a long time, by the great bulk of the profession. When it is finally

acknowledged to be false, the whole profession has to gradually unlearn the lie; this mostly takes a new set of text-books, and, alas! a new crop of practitioners. Nothing is accepted as more than a new-fangled fancy till it appears in the text-books as an unquestioned truth; so that, when a lie so appears, it is easy to see how long it will take to undo the mischief caused. Simple ignorance is conscious of its want, and stands still; error thinks it knows the road, and rushes against fatal obstructions.

Campbell anticipated Tait in his idea, as, at page 152 of his work, he says: "As the placenta, when long retained, is destroyed during the suppurative process, except in some rare instances, and removed from the abdominal cavity with the other decomposed structures, or cannot be discovered, this discloses to us the important fact that the retention of the mass may be permitted without any detriment to the parent, while it can scarcely be doubted that the irritation, which could not fail to be produced by groping for it among the abdominal viscera, or the hemorrhage arising from its detachment, might be succeeded by formidable effects. At one period, it was supposed that the placenta could not be suffered to remain in the abdominal cavity with impunity; but it may be asked, Can the retention of the mass be more injurious to the patient than that of a full-grown fœtus, which, as we are now aware, may remain in the abdominal cavity for a long series of years without any injurious effect?"

Supposing we do not care to do the operation in the way proposed by Tait, we can do it in the ordinary manner, except that it is very important to avoid the linea alba, as, no matter how decidedly the case may be extra-peritoneal, if we *open at* the *linea alba*, we *are sure to open the peritoneum*, thus adding greatly to the danger of the operation. No doubt, this is the reason why the fact, that most cases are extra-peritoneal, has failed to become generally known. The custom is to make the incision in the middle line, and it so happens that the peritoneum is stripped off from behind and below forward, so that in every case there is a strip of peritoneum left attached in front, the breadth of the uterus, and reaching down to the base of the bladder. If the middle line is incised, the peritoneum must be opened, and if a man sees that he has opened the peritoneum to get at a tumor, that, usually, settles the matter in his mind as to its being intra-peritoneal. So, no wonder the belief in "abdominal," or rather "intra-peritoneal,"

pregnancy has gone on so long. Then there can be no doubt that Tait is right on the

Management of the Placenta; that is, that it is much safer to leave it, as a rule. Thomas, Hicks, Barnes, Koeberlé, all agree on this point. Dr. A. Martin, of Berlin, indeed (*Brit. Med. Jour.*, Feb. 11th, 1888, p. 290), advocates its removal, and has had a successful case, and Tait has removed it twice, saving both mothers and both children ("Lectures on Ectopic Pregnancy," p. 96). But I think Tait proves that the dangers of removal in most cases more than counterbalance the dangers of leaving it, even as has been the case hitherto, when decomposition of the placenta has been invited by leaving the wound open. Even if we should prefer drainage of the cyst to hermetical closure, I fail to see what good can be gained by leaving the umbilical cord to rot in the cyst and in the wound; as it, at least, is easy enough removed, why not remove it? The drainage tube will keep the wound open, if we wish that. Now, Lusk states that the placenta is found in the line of incision into the abdomen in one case in six ("Midwifery," p. 339). I fancy this must be a mistake, unless we are to understand it as a mere tail or border of the placenta. The reasons for this belief are—1st. That Dr. James Braithwaite, of Leeds (*Brit. Med. Jour.*, Jan. 3d, 1885), in describing a case where operating, as usual, in the middle line, he came on the placenta as he had beforehand diagnosed, he expressly says that "this *situation* of the placenta *is rare*, and I think it may without much difficulty be diagnosed by the thickness of the structures intervening between the fœtus and the examining hand." 2d. Tait ("Lectures," pp. 83, 84) "The placental relations are *always chiefly pelvic, generally wholly so.*" Again, speaking of Jessop's case, Tait says, "The placenta was found *where it is found in the great bulk of broad ligament cysts*, plastered over the pelvic contents." Besides, it is well known that the placenta is very often implanted into the *back of the uterus*, the peritoneum having been stripped off first. This is one reason why the uterus does not undergo involution until after the child has been some time dead. Of course the placenta is thinner and more spread out than in normal pregnancy, and may be widely implanted, but it is assuredly not the case that the central parts, or body of the placenta, are at all commonly planted on the anterior abdominal wall. The disadvantages of leaving the placenta in primary laparotomy are (1) the tedious and protracted

convalescence; (2) the danger of death from hectic and exhaustion, owing to long-continued suppuration of the sac and its contents during the process of disintegration and throwing off; (3) the possibility of secondary hemorrhage, such as occurred in a case reported by Braxton Hicks. Parry states that the frequency of secondary hemorrhage after spontaneous separation of the placenta, as compared with that after its forcible separation, is one to five. Still, one principal point of greater safety in the "secondary" laparotomy is that bleeding is not to be so much feared. I introduce this matter for the purpose of pointing out to country doctors that the operation of laparotomy, as done by the best surgeons, is one of extreme simplicity, and that no man who has received a medical education need fear to undertake it if he has only reflected on how he would proceed beforehand. The best surgeons don't attempt to remove the fœtus, and they don't attempt to remove the sac, so that, unless there is intestinal obstruction, the adhesions and their separation do not concern him, and certainly, in the majority of cases, he should never ascertain their presence, or give his head any trouble concerning them. All he has got to do is to cut down on the cyst, extra-peritoneally, if he can, or if he can't avoid the peritoneum, then he should sew the edges of the cut, or ruptured cyst, to the edges of the abdominal wall where incised—that is all the extra trouble, if he unfortunately opens the peritoneum. Having incised the abdomen and then the cyst, he has only to remove the fœtus, clean out the sac, cut off the cord, and close his wound either *completely*, as Tait will do for the future, or leave the lower part open and put in a large glass drainage tube, in the old style. If he adopt the drainage plan, he should keep the interior of the sac as dry as possible by sucking out all effusions several times daily with a syringe. It is the *fluids* of decomposition that are especially dangerous, the solid parts of a decomposing mass can only prove harmful by evolving gases which can be absorbed.

Perhaps I should say a few words on the

Treatment of Interstitial Ectopic Pregnancy, should it happen to be diagnosed. We have seen that, as a rule, it is absolutely impossible to diagnose it from tubal pregnancy, or pregnancy in one horn of a bi-lobed uterus. It is probably very rare. There are only five or six specimens in all the British museums. Mr. Alban Doran has stated that there are two in the museum of London Hospital, but Tait says

these are evidently broad ligament pregnancies Parry has 31 cases of interstitial pregnancy in his 500 extra-uterine gestations, but we have seen that, contrary to the maxim that "figures cannot lie," they are peculiarly liable to do so in effect, so we can put no trust in this statement, although if admitted it would support the idea that interstitial cases are infinitely rare. But if a case could be diagnosed before rupture the treatment would be to dilate the cervix, divide the septum in the wall of the uterus and empty the pregnant cornu. It would be of no benefit to kill the fœtus, as the placenta would probably grow sufficiently to burst the cornu unless it were very fully developed. If not, the fœtus, in all probability, would decompose, as it does, sooner or later, in nearly all cases of "*missed labor*," as well as of extra-uterine pregnancy, and in the supposed case the results of decomposition, besides poisoning the system even while in the cornu, would probably be poured into the cavity of the peritoneum by bursting of the cornu. When a cornual pregnancy bursts, prompt opening of the abdomen and hysterectomy, or removal of the ruptured horn, would probably save life, or at all events offer the only chance.

VAGINAL EXTRACTION OF THE CHILD IN EXTRA-UTERINE PREGNANCY.

This is an alternative operation to laparotomy at any period of the extra-uterine gestation, but especially at the full period or after fœtal death. We have shown that it is more dangerous than abdominal section in the early stages of extra-uterine gestation, and we hope to make it equally clear that it is equally objectionable in advanced gestation. It looks, like the plunging of a trocar into the tumor, a much easier and safer proceeding, and, like puncture, it is the resource of timidity and bad surgery, One argument is enough, I think, to exclude it, viz., the fact that the placenta is usually in the pelvis, so that the incision would usually have to go through it, and even worse, the child would have to be dragged through the highly vascular site of the placenta, which, in itself, would usually be enough to turn the scale against the patient. Of course, it has been done with success; what operation has not? but the question is, is it the best, the easiest, the most rational and, in the long run, the most successful operation? I think it can easily be proved that it is none of these. It was done with success, both as regards mother and child, as early as 1817, by

Dr. John King, of Georgia. Campbell reports nine cases, with the saving of five mothers and five children. Parry, up to his time, made a collection of fifteen cases, with six recoveries. But recently it has fallen out of fashion and favor. Billroth, in his "Handbuch der Frauenkrankheiten," 5ter Abschn., p. 87, relates a case where Bandl, in 1874, operated by vaginal section under what he supposed to be most favorable circumstances; the death of the patient followed on the third day. In former times, it was thought to be a great point in the woman's favor, if the fœtus could be easily felt from the vagina, so that all the operation called for was incision of the vaginal wall and application of the forceps. It was thus that Dr. John King, of Edisto Island, operated in 1816 or 1817. Indeed, if the tumor could be reached from the vagina, operation was looked on as quite legitimate.

But elytrotomy, or vaginal section, has been done in the early stage of extra-uterine pregnancy, as well as at full time or afterward, as in Thomas' famous case (*New York Med. Jour.*, June, 1875), where he opened the gestation cyst, previous to rupture, with a platinum knife made incandescent by the galvano-caustic battery, by which he hoped to prevent hemorrhage. Through this incision in the vagina he removed the fœtus, and attempted to remove the placenta, when such violent hemorrhage occurred that he had to inject the cyst with solution of persulphate of iron. The placenta decomposed, and was discharged as débris, the decomposition causing septicæmia, which nearly cost the patient her life, although he freely washed out the cyst with antiseptics. We have already criticised this case, the results of which operation could hardly ever equal Tait's 4.7 per cent. mortality with abdominal section. There is another case of elytrotomy done at about the period of primary rupture, which gives a good idea of the procedure.

In the *Meditz. Obozr.*, Fasc. vii, 1885, p. 689, Dr. Maria O. Prujanskaia, of Moscow, gives the case of a patient, aged 29, who had been twice confined naturally, the last six years ago. She aborted in August, 1884, and again ceased to menstruate in November, when morning sickness, vomiting and the usual breast symptoms of pregnancy appeared. One day, at the commencement of December, as she stooped to put on her boots, she was seized with very severe pains in the abdomen, then giddiness, sickness, dyspnœa, fainting fits and sanguino-

lent vaginal discharge appeared. In the middle of December, and in the beginning and middle of January, 1885, similar attacks occurred. During the fourth attack, the author found the uterus slightly enlarged, softened, the fundus slightly deviated to the right by a firm, elastic, indistinctly fluctuating tumor, which was situated in the left side of the pelvis, and could be felt through the left arch of the vagina. The cervix uteri was patulous. Various common signs of pregnancy were present, and the author diagnosed a three months' extra-uterine gestation. The peritonitic symptoms with prostration, which, evidently, were caused by primary rupture, were put down to "partial rupture of the superficial layers of the fœtal sac accompanied by hemorrhage into the peritoneal cavity." What are the superficial layers of the fœtal sac but the walls of the Fallopian tube? The author proposed to kill the fœtus by morphine injections or by electricity. As these were declined, nothing of any import was done. At the end of January, fœtal movements were distinct through the abdominal walls. In the middle of February, a fifth attack of peritonitic symptoms caused the patient to become submissive to any treatment. Professor Snegireff diagnosed a left tubal pregnancy *at the point of rupture*, and performed elytrotomy by Paquelin's thermocautery; an incision about three fingers' breadth long into the roof of the vagina permitted easy podalic extraction of a live fœtus of four and a half months. The umbilical cord was tied and divided, the placenta left *in situ*, the sac and vagina washed out with sublimate solution, and the cavity of the sac and the vagina filled with iodoform gauze. The operation was almost bloodless (the uterine artery had been tied immediately after the incision), and lasted only ten minutes. Death occurred the same night. No *post-mortem* examination was obtained. Now, here we have a case where elytrotomy was done in the most scientific way, where the placenta was not interfered with, where the most powerful antiseptics were used, where there was no hemorrhage to lay the blame on, and yet the result is speedy death.

A successful case at term, where the child and mother were both in good health years afterward, was related to the London Obstetrical Society by J. H. Matthieson, of St. Mary's, Ontario, on May 7th, 1884 (*Brit. Med. Jour.*, 1884, p. 999). As the course of the extra-uterine pregnancy down to the false labor was typical, I need not burden my pages with it, but merely describe the mode of operation

pursued. It was done without chloroform, by cutting with an ordinary knife, and tearing with the finger-nail. The face presented, but the child was easily extracted with forceps. The placenta was at the *posterior and left side of the cavity*, its lower border some two inches from the vaginal roof. It was easily peeled off, a sponge soaked in perchloride of iron following it up. There was very little bleeding. The clots were removed from the cyst, and its walls touched with perchloride. Much exhaustion followed. The placenta was tri-lobed. The cyst was washed out antiseptically after operation. The wound healed in three months. The patient had severe pain in the side for two years after the operation. Reading of the case was followed by debate. Dr. H. Gervis (President) doubted the propriety of the vaginal operation and of the removal of the placenta. Each case must be judged on its own merits, but, as a rule, he favored primary laparotomy and leaving the placenta. Dr. Playfair fully agreed with the President. Dr. Edis also advocated primary laparotomy, owing to the advances of abdominal surgery (in 1884). Dr. Griffith thought that the presence of a contractile cyst wall necessitated the gestation being either interstitial or in an undeveloped form of a double uterus. Dr. Champneys said that the uterine *platysma*, which extended widely over the pelvic organs, was sufficient to account for contractions, and that these occurred in undoubted extra-uterine pregnancies.

Dr. Herman, Obstetric Physician, London Hospital, has arrived at a number of important conclusions with regard to vaginal section, which he communicated to the Obstetrical Society of London at a special meeting for the discussion of the treatment of extra-uterine gestation during the later months of pregnancy. The meeting was on November 23d, 1887, and is reported in *Brit. Med. Jour.*, December 3d, 1887, p. 1213.

Dr. Herman said no general rules could apply to all cases and at all periods of gestation. He gave a case of his own in a patient aged 40. The extra-uterine pregnancy was preceded by a long period of sterility. Symptoms like those of rupture occurred at about two months' gestation. At nine months false labor occurred, and lasted nearly a month. The labor pains were accompanied by dilatation of the cervix. Then the pains ceased, the breasts became smaller, and the cervix contracted. The cyst was opened by vagina, the child removed, and carbolic washings of the cyst freely used. The placenta

came away on the sixteenth day. Cyst closed in 2½ months. Dr. Herman has collected thirty-three cases of elytrotomy for extra-uterine gestation, and from an analysis he drew the following conclusions :—

1. The operation by the vagina, previous to rupture, by the cautery knife or otherwise, is a *dangerous and unscientific* proceeding. Abdominal section ought always to be preferred.

2. Abdominal section is more likely to succeed than vaginal, after tubal rupture with hemorrhage threatening the life.

3. After primary rupture, when hemorrhage is followed by fever, the indications for vaginal incision are just the same as in hæmatocele from any other cause. (Tait agrees with this.)

4. At or soon after term, before suppuration has occurred, there may be conditions which indicate vaginal delivery as preferable to abdominal section. These are :—

5. *When the child presents by the head, breech, or feet, so that it may be extracted without turning ;* and,

6. *When it is quite certain, from the thinness of the structures separating the presentation from the vagina, that the placenta is not in the septum, and when it is not certain that the placenta is not on the anterior abdominal wall.*

7. If the child cannot be delivered without turning, prefer abdominal section.

8. No attempt should be made to remove the placenta.

9. Frequent washing out of the sac should constitute the after treatment.

10. After suppuration has occurred, the natural opening of the sac into the vagina is one of the more favorable probable terminations.

These are not Herman's exact words, but they contain the substance and spirit of his conclusions. Herman's only ground for preferring vaginal section in any case, seems to be the fear of meeting with the placenta on the anterior abdominal wall. Now, I think I have shown that the pelvis is the seat of the placenta in the vast majority of cases ; that the abdominal wall is a very rare site, and that it is not impossible to diagnose its presence there before operation. Dr. Braithwaite, of Leeds (*Brit. Med. Jour.*, Jan. 3d, 1885), met with a case of abdominal placenta, but he is of the opinion that it is rare and easily diagnosed. And in his case the incision (in the median line) was only two inches from the edge of the placenta, and it gave no trouble.

"Careful separation of the placenta in this direction did not produce hemorrhage." Lusk estimates that the placenta is on the anterior abdominal wall in one case out of six; but this is probably a mistake, made by raking up old statistics, as it is decidedly contrary to Tait's opinion, who has more personal experience than any living man. At the debate on Herman's conclusions, Mr. Knowsley Thornton made several weighty objections to Dr. Herman's conclusions. With conclusions 1 and 2 he agreed. He thought 3 was hardly precise enough. As to vaginal extraction, he doubted whether, in most cases, the diagnosis could be as easily made as Dr. Herman indicated, and he did not think the risk of *injuring a coil of intestine* was a small one, though this was not mentioned by Dr. Herman. He thought, with the present means of stopping hemorrhage, the possibility of the placenta being situated on the abdominal wall *need not be such an alarming complication* of abdominal section *as to influence the choice of operation.* The whole question of primary laparotomy hinged on perfect diagnosis, and knowing how often there was something wrong with the child in these cases he would disregard it altogether, and simply consider the mother, and urge that *operation should follow at once on certain diagnosis.* Lawson Tait said he quite agreed with Mr. Thornton as to the difficulty of diagnosis, but he never allowed any uncertainty to stand in the way of trying to serve his patient, and in doubtful cases he opened the abdomen. Dr. Herman in reply touched on two very important points: 1. The fact that in some cases it is extremely easy to make the diagnosis from the vagina, when the placenta is not between the presentation and the examining fingers. He said we could often feel the sutures of the skull; in that case he thought the intestines safe. 2. The condition of the placenta; Dr. Herman pointed out that there are two distinct kinds of placenta in extra-uterine gestation at an advanced stage. The first was the common variety, where it was thin and spread out very much, and where it was both difficult and dangerous to remove it in either operation. In the second variety the placenta was a thick, solid mass, with very narrow attachment. These are the placentæ of which we hear occasionally, which are removed in both forms of operation so easily. He believed the spreading out often occurred by growth and sprouting of the placenta *after fœtal death.* If Dr. Herman is proved to be right on this point, it would be an additional argument for primary laparotomy.

But it is Lawson Tait who is the great opponent of vaginal section, and in dealing with Herman's conclusions, he says that, admitting them all to be perfectly correct, in Nos. 5, 6, and 7 Herman lays down objections which will exclude the vaginal operation; for, after fœtal death (and the great majority of cases only present themselves when their supposed pregnancy has not been followed by delivery), it is *absolutely impossible to tell where the placenta is*, nor is it always certain even while the child is living. Tait declares he has twice failed to discover its seat, even with his hands in the fœtal sac ("Lectures," p. 84). Tait also believes that the most expert obstetricians could not accurately ascertain the presentation of an extra-uterine fœtus until the sac had been opened—at least, he once saw a very skillful man fail. Tait says that Dr. Herman has collected a series of twenty-three cases of elytrotomy (the *Brit. Med. Jour.* reports Herman as saying thirty-three), with *fourteen* mothers and only *one child saved.* Tait has searched out several others, but he says this kind of research is really without value, for there are so many points of discrepancy that it is utter folly to argue from such tables to any general, and much more any particular, conclusion. That elytrotomy is very unsatisfactory as to the child is proved by the constant difficulties experienced in removing the fœtus, and by the fact that there are only two cases where it survived for any length of time. Tait says the mortality of all the collected cases is over 60 per cent.; but he says this is not half so good an argument against the operation as the reports of the tearing of the parts which was revealed at the post-mortem examinations, and the concealed hemorrhage which was almost always the real cause of death. He says he has done it once, but shall never, under any circumstances whatever, operate on an extra-peritoneal pregnancy from the vagina. In his case, reported at length in *Med. Times and Gazette*, 1873, the patient had been allowed to run down—the great cause of the high mortality of all these operations; and although he easily delivered an eight months' fœtus which had been dead some time, and even removed the placenta without the least hemorrhage, she died in a few hours. He says that, in this instance, the temptation to resort to elytrotomy was great, for the fœtus felt as if only separated from the finger by mucous membrane. It seemed as if a scratch of the nail would free the child, and he thinks this was the temptation to most who have done the operation, but he says it is clearly wrong for two reasons: 1. The placental relations are

always chiefly pelvic, *generally wholly so*, the child must, therefore, be dragged through tissues in which large sinuses have been abnormally developed, tearing and damaging them, and with every probability of killing both mother and child. And if, as so often happens, there be bleeding from the torn maternal sinuses, it is quite hopeless to find them, or secure the bleeding points. Tait concludes that opening the peritoneal cavity, where it must be opened, is a "clumsy and risky procedure under *any* circumstances," and has no advantage over the abdominal method, while it has many disadvantages. But unless people will persist in making the incision in the middle line of the abdomen, there is really no necessity for opening the peritoneum in many cases, at least. This fact is exemplified in a case reported in the (*Nouv. Arch. d' Obstet. et de Gyn.*, Feb. 25th, 1887) practice of Dr. Busch, who did the right thing without probably knowing anything about the rearrangement of the peritoneum which occurs in broad ligament pregnancies, as proved by Tait, Berry Hart and Carter, and which makes the fœtus so easy to get at unless we go to the middle line of the abdomen, where the peritoneum is never peeled off. In the case of Dr. Busch, the patient was aged 27, and had already been delivered of a child. The present gestation had gone on normally until between the second and third month. She had then a good deal of abdominal pain. At the onset of false labor extra-uterine gestation was diagnosed. It was only eight months later, however, that an operation was decided on. An incision four centimètres in length was made above and *parallel* to Poupart's ligament, and as soon as the *skin was cut through* the fœtal head was seen. Craniotomy was performed and the fœtus extracted in bits, so that it might be unnecessary to enlarge the incision. The cyst was washed out with boracic acid solution, and recovery was rapid and complete.

Owing to the author's residence abroad, it has not been possible for him to either revise this essay, or to correct the proof sheets.

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